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PHILOSOPHICAL TRANSACTIONS.

CORRECTION TO MR. G. H. DARWIN'S PAPER IN THE PHILOSOPHICAL TRANSACTIONS FOR 1880, P. 713.

For "autumnal equinox" *read throughout* "vernal equinox."

N.B.—This correction does not affect the results of the analysis.

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muscle. In a second paper, which was read on the 14th December, 1876, I submitted to the Society, in conjunction with Mr. PAGE, an account of a number of experiments, made, for the most part in the Laboratory of the Royal Gardens, Kew, with the aid of LIPPMANN'S electrometer, for the purpose of elucidating the relation between the electrical phenomena and the physiological process which they accompany. In that paper we confined ourselves to the mere relation of our observations, recognising their incompleteness, but hoping that the work of future seasons might yield better fruits. Although each year since 1876 some additions have been made to our knowledge, it has not seemed desirable to publish anything until the present occasion. If the accumulated products of so many years appear insignificant, I must ask that it may be remembered that the investigation is one of great difficulty, and that the time during each summer which is available for experiment is very short.

As almost all the experiments on which the conclusions of the present paper are founded have been of such a nature as to require the co-operation of two persons, it is obvious that the work could not have been accomplished without the aid of a practised observer. This aid I have received from Mr. PAGE, to whose technical skill and ingenuity in overcoming difficulties, whatever success may have been attained in the experimental part of the enquiry, is largely due.

I have to express my great indebtedness to the Director of Kew Gardens, Sir JOSEPH HOOKER, and to the Assistant Director, Professor THISELTON DYER, for the invariable kindness with which they have placed the resources of the Royal Gardens at my disposal.

Since 1873 several researches have been published in Germany relating to the excitatory process in Dionæa, or more generally to plant excitability. In 1876 an elaborate work appeared in Berlin by Professor MUNK, of which the aim was to acquire "a more exact knowledge of the leaf of Dionæa in its electrical relations;" and in addition two important experimental papers by Dr. KUNKEL relating chiefly to *Mimosa* have been lately published in vol. xxv. of PFLÜGER'S Archiv, under the title "Electrical Researches Relating to Animal and Vegetable Structures." For the purpose of deriving from them valuable aid, no less than for that of criticising the observations and conclusions of their authors, it will be necessary to examine these papers carefully. I therefore propose to devote an introductory section to this object.

PART I.

EXAMINATION OF THE RECENT RESEARCHES OF PROFESSOR MUNK AND OF DR. KUNKEL ON PLANT ELECTRICITY.

1. *Summary of Professor* MUNK'S *Paper.*[*]—The introduction (§ 1) begins by referring to the discovery, made by myself in 1873, that the leaf of Dionæa is endowed with electromotive activity resembling that of muscle, and that when the leaf contracts a negative variation occurs. He states that his attention was first directed to Dionæa in November, 1873, and that, in consequence, he made experiments in 1874 and 1875, using about 100 leaves, none of which were detached from the plants to which they belonged.

§ 2 relates to the distribution of electrical tension on the surface of the leaf. After confirming the observations recorded in my first communication to the Royal Society, that in general when the Dionæa leaf is led off by the opposite ends of the outer surface of the midrib, the end furthest from the leaf stalk (distal) is found to be positive to the other end[†], he states (1) that the most positive point is not the distal end of the midrib, but the point corresponding to the junction of its middle with its distal third, (p. 37) to which point, though both ends are negative, the petiole end is twice as much so as the distal end. (2) That in any line drawn across the midrib on the outer (under) surface of the leaf from side to side, a point can be found on either side which is negative to all other points, the negative point being situated nearer to the border of the leaf than to the midrib (p. 38), and finally that the distribution of tension is the same on the under surface as the upper (p. 42) and that opposite points are always equipotential.* In § 3 the difference of potential between the most negative and the most positive points of the outer (under) surface of the leaf is found to be 0·04 to 0·05 DANIELL. In § 4 the question of the seat of the electromotive forces on which this and the other observed differences are dependent is discussed.

---

[*] Die elektrischen und Bewegungs-Erscheinungen am Blatte der Dionæa Muscipula, von Dr. HERMANN MUNK, a. o. Professor an der Universität zu Berlin. Leipzig, 1876. Aus REICHERT'S und DU BOIS-REYMOND'S Archiv für Anatomie, &c., 1876, besonders abgedruckt.

[†] "Herrn SANDERSON'S Angabe ist leicht zu bestätigen: man findet in der That regelmässig zwischen jenen Blattenden, nach unserer Bezeichnungsweise, einen aufsteigenden Strom."

From preliminary considerations he concludes that inasmuch as it is out of the question "das Blatt zu zerstückeln" so as to investigate the electromotive properties of its parts separately, the only way is to start from the well grounded assumption that wherever the same organisation exists there will be similar arrangement of electromotive forces, and accordingly that in the individual cell the electromotive force will show a distribution which has a definite relation to its long axis. Hence, inasmuch as the form of the cell admits of but few possibilities, we may, by ascertaining which of these possibilities when taken in relation with the known arrangement of the cells in the leaf, affords the best explanation of what is actually observed in the distribution of electrical tension on the surface of the leaf, select that which accords best with fact. In order to get at this result Professor MUNK employs a method of investigation which I shall not endeavour to describe in detail, but content myself with stating that it is founded on the assumption that it is possible to represent the living cell, as regards its electromotive properties, by a zinc cylinder having a zone of copper and surrounded by a uniform layer of a moist conductor;† and that a schema or model made up of a number of such cylinders arranged like the cells of any living part of a plant would exhibit electromotive properties similar to those of the organ it represented. Stated as shortly as possible, the actual arrangement of the parenchyma cells of the leaf of Dionæa is as follows: in the lobes the cells run all in one direction, namely, parallel to the veining and at right angles to the midrib. In the midrib the cells of the ridge (under surface) run longitudinally, but those of the trough (upper surface) are continuous with those of the cells of the lobe. For reasons which need not be here entered on (see p. 86) Professor MUNK considers that the arrangement which determines the electromotive properties of the leaf, is that of the cells of the lobe-parenchyma of which the long axes are parallel to the surface of the lobe and at right angles to the midrib. Accordingly, the leaf ought, on the assumption that the cells have the properties assigned to them (*i.e.*, that the middle of each cell is negative to its ends), to be represented by a bilateral schema consisting of two symmetrical parts, resembling in form the two lobes of the leaf, and each made up of metallic cylinders arranged like the cells in the leaf, as above described. A full account of the properties actually demonstrable in such an arrangement is given in the paper (pp. 54 and 55). It corresponds in the most important respects, as regards the distribution of electrical tension, with Professor MUNK's account of the electrical properties of the resting leaf.

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* "An der oberen Blattfläche die gleiche Vertheilung der Spannungen herrscht, wie an der unteren Blattfläche" (p. 42). "Dass die absolute Grösse der Spannungen an der oberen und an der unteren Blattfläche die gleiche ist" (p.).

† "Die einzelne Parenchymzelle entspricht hinsichts ihrer Kräfte dem einzelnen Metallcylinder" (p. 96). DU BOIS-REYMOND's schema of an electromotive element of muscle is a copper cylinder with a zinc zone surrounded by a uniform layer of a moist conductor. For the plant electromotive element, according to MUNK, the sign must be reversed. The cylinder must be zinc and the zone copper.

§ 5 relates to the "Mechanism of the Excitatory Motion." On this subject it will be sufficient to state that the account given by Professor MUNK of the way in which the leaf closes when touched, agrees with the explanation originally given by BRÜCKE of the mechanism of the motion of the sensitive plant; namely, that the immediate cause of the change of form is the sudden passing of certain cells (in the case of Dionæa those of the upper (inner) layer of the lamina and midrib) from the state of distension into that of flaccidity; but, in addition to this, there is, he says, an active lengthening and extension of the cells of the lower or external layer.* From the context it appears that all that is here meant is that the water discharged in the sudden relaxation of the cells of the inner (upper) stratum, finds its way at once to the outer (lower). At all events, the only evidence given in support of active extension is the fact, proved by measurement, that two marks on the external surface of the expanded leaf, in a line parallel to the veining and at a measured distance from each other, are found to be further apart after the leaf has closed (p. 117).

In § 6 (p. 123) the author approaches the question which more immediately concerns our present purpose, viz.: the electrical phenomena which result from excitation. I will endeavour to give an account, first of the experiments, and secondly of the theoretical explanations—premising that, in this as in the section on the electromotive properties of the unexcited leaf, observations and theories are so mixed that it requires much patience to separate them.

The mode of experiment was always the same. The leaf was led off, either by different points on the under surface of the midrib, as in my original experiments in 1873, or by the midrib and by a spot on the external surface opposite to it. The leaf was not fixed or restrained, so that only a very few excitations could be observed before closure.

All of the experimental results are stated in the following table, in the first column of which I have entered the leading off contacts, distinguishing them by the letters *A* and *B*, in the second column whatever information is given as to the difference of potential between them before excitation. The third column gives the character of the excitatory effect as observed by the galvanometer; and the fourth, the reference to the page where the experiment is recorded.

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* "Die Bewegung kommt dadurch zu Stande dass das reizbare Parenchym erschlafft und kürzer wird, das nicht reizbare sich activ verlängert" (p. 119).

TABULAR Summary of the Results of Professor MUNK's Experiments.

	Contacts.		Electrical condition before excitation.	Excitatory effect.	Reference.	Remarks.
	A.	*B.*				
I.	Distal end of midrib.	Proximal end	Aufsteigender Strom, *i.e.* *A* 30 scale negative to *B*.	"Negativer Vorschlag," *i.e.* diminution of previous deflection, followed by "positive Schwankung," *i.e.*, increase of previous deflection.	P. 128 . . .	Electromotive force of the "positive Schwankung" said to be about equal to that of "aufsteigender Strom."
II.	Distal half .	Proximal end	Not stated; in general, the difference of potential said to be twice as great as in the previous case. (P. 38, line 7.)	"Neg. Vorschlag" so strong as to reverse the previous deflection, and to exceed the Schwankung several times in extent.	P. 131 . . .	Relation of Vorschlag and Schwankung (Doppelschwankung) to previous deflection inconstant.
III.	Distal half .	Proximal half	Contacts isoelectrical .	First effect "aufsteigend," *i.e.* in direction of the Schwankung, followed by slow swing in opposite direction, and return to zero.	Pp. 131, 132.	
IV.	Under surface of midrib at junction of middle and distal third.	Outer concave surface of lobe opposite *A*, and at most negative point.	*B* negative to *A*. *Difference* amounts to 0·05 DANIELL.	Vorschlag of ·2 to 3 scale, in which *B* becomes less negative, followed by very large deflection in the opposite direction.	P. 145 . . .	In this experiment the leaf is led off by the two points of which the potentials differ most.

Professor MUNK's explanation of the *excitatory variation* is, of course, in conformity with his doctrine of the electromotive activity of the individual cell, which has been already explained; namely, that each parenchyma cell has the endowment of a cylindrical electromotive element of which the poles are positive to the middle,* and the excitatory change produced in each cell consists in increase or diminution of the electromotive force of each cell. If the variation consisted of a single phase, it might be accounted for by supposing that all the cells of the parenchyma of the lobes are affected by excitation in the same way. As, however, it presents the characters of a "*Doppelschwankung*"—*i.e.*, consists of two phases succeeding each other, of opposite signs, this must be accounted for either by supposing (1) that all the electromotive elements in the structure are similarly affected, but not at the same time, the excitatory change being propagated from the seat of excitation in such a way that it first affects the electromotive elements at one contact, then those at the other; (2) that all the electromotive elements are similarly affected at the same time, changes of opposite signs following each other in each element simultaneously; (3) that there exist in the structure two sets of electromotive elements, which are affected in opposite directions by excitation, the disturbance reaching its maximum in the one set later than in the other set.

Of these three alternatives only the third appears to Professor MUNK admissible. The first he rejects on the ground that if it were true the character of the excitatory variation would be affected by the seat of excitation. This he finds not to be the case. Thus in Exp. II. (see Table) the effect should be different according as the leaf is excited by touching the proximal or distal hairs. According to Professor MUNK there is no difference (p. 136) either in this or other similar cases. He therefore concludes that the "*Doppelschwankung*" is not due to propagation of the excitatory effect from the seat of excitation. "Danach kann weder die complicirte Schwankung in irgend einer Beziehung zum Orte der Reizung stehen, noch kann überhaupt von diesem Orte der Erfolg der Reizung, innerhalb der Genauigkeitsgrenzen unserer Untersuchung, irgendwie abhängig sein" (p. 138). The second alternative is rejected on the ground that in IV. (see Table) the Vorschlag, if it occurs at all is very inconsiderable, and is very frequently absent, consisting of one phase only, which could not be the case if each cell underwent first a diminution then an increase of its electromotive force, and all acted simultaneously. The third alternative, thus adopted by exclusion, viz.: that the two sets of electromotive elements are oppositely affected by excitation, serves, Professor MUNK thinks, to explain the characters of the excitatory effect in all the cases observed by him.

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* "Die ohngefähr cylindrischen Zellen des Blattflügel-Parenchyms und der beiden Mittelrippen-Parenchyme sind mit Kräften ausgestattet der Art, dass die positive Elektricität von der Mitte der Zelle nach jedem der beiden Pole hingetrieben wird, die Pole positiv sind gegen die Mitte" (p. 97).

Professor MUNK'S paper does not contain any statement of his conclusions. For further reference I have arranged them as follows, in the order in which they have been referred to in the preceding sketch:—

1. Each individual parenchyma cell of the leaf corresponds, in respect of its electromotive properties, with a zinc cylinder zoned with copper and surrounded by a uniform layer of moist conductor. In other words, the nearly cylindrical cells are endowed with electromotive forces such, that positive electricity is driven from the middle of each cell towards each of its poles, *i.e.*, the middle is negative to the poles (p. 97).

2. The electromotive properties of the whole leaf (pp. 47–51) in the unexcited state are represented by those of a schema (see diagram, fig. 20, *a*), consisting of such cylinders arranged in the same order as its cells (p. 87).

3. On excitation, the cells of the upper layer of the leaf undergo a diminution of electromotive force, those of the under layer an increase, *i.e.*, the middles of the cells become less negative than before in the upper layer, more negative in the lower layer (p. 152).

4. The diphasic character of the variation is not due to the fact that the individual cells undergo opposite changes in succession, but to the interference of the opposite electromotive actions of the upper and under cells, of which the time relations are different (see diagrams 29 and 30 and p. 142).

5. The effect of an excitation is in no way dependent on its seat (p. 138).

6. The electromotive activity of the cells has no relation to their water-content (p. 158).*

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* The author's criticisms of my methods and conclusions will be best considered in connexion with the several subjects to which they refer. His paper contains some misconceptions which it will be convenient to correct in this place. One of the most important, as having led other writers into error, is founded on a casual expression of mine contained in a lecture at the Royal Institution in 1874, to the effect that the excitatory motion of the leaf is identical with muscular motion. No such expression is contained either in my communication to the Royal Society, or in the translation of it which appeared shortly afterwards in the 'Centralblatt.' ("Ueber electrische Vorgänge im Blatte der *Dionæa muscipula*." 'Centralblatt f. d. Med. Wissensch.,' 1873, No. 53.) If Professor MUNK had considered the context, he would have seen that in my lecture I meant nothing more than that both motions are expressions of the same elementary endowment of protoplasm—that of changing its form on excitation. I held then, and hold now, that, as regards what happens in the living protoplasm, the mechanical effects of excitation in the contractile parts of plants and animals are closely related. As to the mechanism by which the changes of form of the excitable organs of plants are brought about, nothing was said in my paper. I took for granted the explanation usually given, founded on BRÜCKE'S experiments. I had not become acquainted with the then recently-published researches of PFEFFER on the excitability of plants ('Physiologische Untersuchungen,' von Dr. W. PFEFFER. Leipzig: 1873).

As regards the original experiments by which I demonstrated the fact that the leaf of Dionæa responds to excitation by an electrical disturbance analogous to the excitatory variation of muscle, Professor MUNK'S

The researches of KUNKEL,* like those of MUNK, have been directed to two questions, viz.: to that of the differences of electric tension which present themselves in the uninjured leaf, and to the electromotive phenomena which follow excitation in the excitable organs of plants. As regards the first question, he has found that in leaves generally, when they are led off by non-polarizable electrodes in the usual way, differences of tension present themselves, these being always of such a nature that if one electrode is in contact with a vein, the other with the green surface between the veins, the former is positive to the latter (p. 359). But he considers this due, not to any real difference of tension between the surfaces, but exclusively to inhibition changes which begin at the moment that contact is made. As evidence that the relations of the two surfaces to water are different, he adduces the following observations.

That diffusion actually takes place at the uninjured surfaces of leaves which are in contact with water, he proves by the observation that potassium salts may be detected

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observations are, happily, in accordance with mine, as may be seen by comparing his account of the phenomena with mine:—

SANDERSON, 1873.	MUNK, 1876.
Wenn das Blatt so auf die Elektroden aufgelegt wird dass der normale Strom des Blattes durch eine Ablenkung der Nadel nach links angezeigt wird und die sensitiven Haare der oberen Fläche berührt werden, so schwingt die Nadel nach rechts. . . . In jedem Falle kommt die Nadel nach der negativen Schwankung in einer Stellung zur Ruhe, die weiter nach links gelegen ist als zuvor, und nimmt dann allmählig ihre frühere Stellung wieder ein. (Centralblatt, 1873, p. 835.)	Reizen wir unser Blatt bei Ableitung von den beiden Enden der Mittelrippe, so beobachten wir Folgendes: (Es sei z. B. eine Ablenkung von 30 Sc. vorhanden.) Zuerst nach der Reizung nimmt die Ablenkung sehr rasch auf 25–30 Sc. ab, und darauf nimmt sie sogleich etwas langsamer, doch immer sehr rasch auf 40–60 Sc. zu. Nun verweilt der Spiegel nur momentan auf dem Maximum der Ablenkung und kehrt dann langsam, in etwa einer Minute, zu seiner Anfangsstellung zurück (pp. 127, 128).

Notwithstanding this close correspondence, Professor MUNK speaks of my description as grossly defective, "not merely in details, but in what is essential and important" (p. 124); and proceeds, that for aught I knew to the contrary, my "*negative Schwankung*" might be nothing more than a result of shifting of contacts. The serious defect, however, was not so much incompetence as an observer as inability to understand what I saw; for, referring to the very same observation further on, he says that "Herr S. den Erfolg der Reizung richtig beobachtet hat und nur seine Beobachtung durchaus missverstanden hat, indem er . . . eine negative Schwankung da zu sehen vermeinte, wo in der Wirklichkeit eine positive Schwankung mit negativem Vorschlage sich zeigte." On another of my observations (see p. 152), which he had not time to repeat, but which he thinks may be relied on though it was made "natürlich ohne von seiner Bedeutung eine Ahnung zu haben," he finds an additional prop for his own theories. In conclusion, Professor MUNK congratulates himself on having substituted "an die Stelle der Spurweisen und vielfach unzutreffenden ersten Wahrnehmungen von Herrn Sanderson eine genauere Kenntniss des Dionæa-Blattes in elektrischer Beziehung."

* KUNKEL. "Electrische Untersuchungen an pflanzlichen und thierischen Gebilden," PFLÜGER'S Archiv, Bd. xxv.

in water in which such leaves have been immersed even after a few minutes' immersion. He further finds that if a freshly cut off leaf is immersed in water, the whole of the vein surface is wetted at once. The green surface of the lamina exhibits a beautiful silvery lustre indicating that air adheres to it, and that water comes very slowly and incompletely into contact with it. When the leaf is removed from the water, the veins remain permanently and completely wet, but the water runs off the rest of the surface, leaving it dry. Consequently he concludes that when moist electrodes are brought into contact with surfaces which thus differ, the diffusion processes which result from the contact must go on at different rates.

By other experiments, physical rather than physiological, he has shown that in all bodies capable of imbibition with water, electrical changes are associated with the process of imbibition. His first experiments on the subject were made with the ordinary porous cells of batteries. Similar results were obtained by the investigation of non-living vegetable structures, as *e.g.*, laminaria stems. By comparing these facts with those already stated as to the electrical phenomena observed in the investigation of leaves, he derived the conclusion that whatever differences present themselves between the different parts of leaves are due to "*Unterschied in der Benetzbarkeit*" of the surfaces led off. What the direction of this effect is, KUNKEL had determined by experiment. If a drop of water is placed on the green surface of a leaf between the veins and allowed to remain there some time and the leaf is now led off by another similar surface and by the drop, it is found that the surface first wetted is always positive to the other (p. 368). If after one electrode has been brought into contact with an ordinary surface, the other is applied to a vein, the direction of the difference is opposed to the normal. The vein is at first negative but the difference quickly diminishes and is soon reversed (p. 360). In other words, when a leaf or other similar structure is led off by two surfaces of which one is more "wettable" than the other, the former is positive to the latter, provided that both contacts have been made simultaneously.

From these observations, of which the value and interest must be recognised as showing what sort of results may arise from imbibition processes taking place at surfaces of contact, the author proceeds to draw inferences as to the nature of excitatory electromotive changes in the organs of plants, confining his attention to *Mimosa.* The excitatory effect in the swelling at the base of the leaf stalk of *Mimosa*, is described by KUNKEL, on the basis of observations made with the capillary electrometer, the organ being led off by thread electrodes applied to its opposite ends as follows. It is said to consist of a "*Vorschlag*," in which the base becomes negative, followed by a larger excursion in the opposite direction. KUNKEL thinks that this observation is in harmony with those of MUNK. Three fragmentary experiments are given in confirmation of this, on which the theory is founded, that inasmuch as in *Mimosa* the active motions of the leaf are caused by a sudden alteration in the distribution of water in the different parts of the swelling at the base of the leaf stalk, that these "*Wasser-*

Wasserverschiebungen" are the causes of the electrical changes observed, the proof being, that in general "*Wasserverschiebungen in imbibirten Körpern*" are attended with electrical phenomena. He confesses himself unable to connect the particular phenomena observed with "*einzelne Phasen prevalirender Wasserverschiebungen.*" He thinks, however, that the first "*Vorschlag*" is not due to *Wasserverschiebung*, but to the fact that the diffusion processes which are called into existence by the application of the electrodes are disturbed by alterations in the protoplasm. It is to be noticed that these explanations, which are contained in the concluding paragraph of the paper, are not in accordance with the more general statement on p. 371. In one place it is stated that "all electrical phenomena are conditioned by *Wasserverschiebung;*" in the other, that the *Vorschlag* in *Mimosa* "cannot be referred to *Wasserverschiebung*, but to the alteration of protoplasm which precedes it." The difference is a fundamental one.

From the preceding pages it will be seen that the two observers who have investigated the electromotive phenomena which are associated with the excitatory process, viz.: Professor MUNK and Professor KUNKEL, have arrived at opposite conclusions as to their nature. According to MUNK, the electromotive activity of the plant cell stands "in no direct relation to the water it contains" (p. 158). It is KUNKEL's main conclusion that they are determined by diffusion processes. MUNK gives as the result of his observations a complicated map of the distribution of electrical tension on the surface of the leaf of Dionæa (p. 40, figs. 14 and 15). KUNKEL starts from the proposition (which he does not stop to demonstrate) that electrical differences do not exist at all in uninjured and untouched leaves (p. 372). According to MUNK the electromotive properties of the leaf are referable to electromotors which resemble the electromotive molecules of DU BOIS-REYMOND, with the exception that their signs are reversed (p. 51). This hypothesis KUNKEL only mentions for the purpose of rejecting it (p. 371). There is, in short, but one respect in which they agree. Their descriptions of the phenomena are sufficiently explicit to make it pretty certain that they have both witnessed the same process. As regards the physiological nature of the phenomena the contradiction is complete.

PART II.

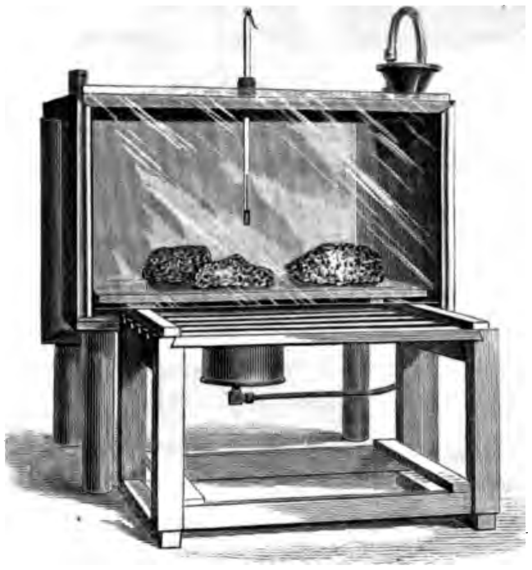
DESCRIPTION OF THE METHODS AND INSTRUMENTS EMPLOYED.

The methods used in the study of the electromotive properties of an excitable living structure, whether of plant or animal, may be grouped under four heads according as they are adapted (A) for maintaining, during the period of observation, the external conditions of temperature, moisture, &c., which are most conducive to vigorous life; (B) for the investigation of the differences of tension (electrical potential) which exist *at one and the same time* between different parts of the surface of the organ; (C) for investigating the *successive* states (tension differences) which present themselves after excitation, with reference to their duration and their time-relations with other phenomena or processes which precede or accompany them; or (D) for exciting, or otherwise temporarily modifying the physiological conditions of the organ.

The appliances used for these several purposes in the case of the leaf of Dionæa are as follows :—

A. *Means of maintaining favourable conditions of temperature, &c., during the period of observation.*—By preliminary experiments it was ascertained that the conditions most favourable to the activity of the leaf are, that the temperature of the leaf should be from 32° C. to 35° C. and that the air should be nearly saturated. For the maintenance of these conditions the plant is placed in a chamber of the following construction (fig. 1).

Fig. 1.



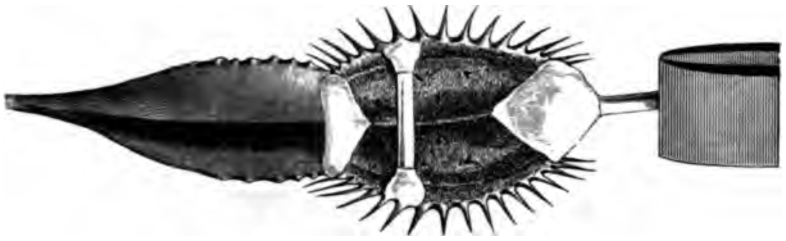
The Warm Chamber.

The sponges which serve to keep the air saturated rest on the glass plate which covers the sliding floor. On this glass plate the plant, electrode holders, &c., rest. To withdraw them, the floor is made to slide out along the rails. The upper part of the tube of the regulator is seen on the right side. (From a photograph.)

The chamber measures 47 centims. wide and 40 centims. from back to front, and is 26 centims. high. It is made of japanned tin plate. Its top, bottom, and sides are double, and separated from each other by a closed cavity of $1\frac{1}{4}$ centims. in width, which is filled with water. Its front and back are closed by panes of glass which slide up and down in grooves. The water contained in the bottom, sides, and top is heated by an ordinary gas burner, 3 inches below the bottom, along which, and up one side a chimney, $2\frac{1}{2}$ centims. by $7\frac{1}{4}$, leads. The temperature of the water is governed by an automatic regulator of the form described by Mr. PAGE in the Journal of the Chemical Society.* The chamber is provided with a sliding floor of wood, covered with glass, which serves to support the apparatus. The wires leading to the electrodes, &c., enter by openings left on either side of the floor for the purpose.

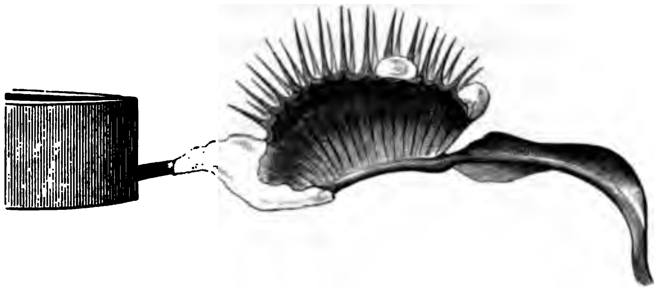
Mode of preparing the leaf for observation.—Whether the leaf is on the plant or not, it is necessary to fix it in such a way as to render it motionless. In our earlier experiments glass forceps were used, ingeniously contrived and constructed by Mr. Page, of which the blades were of such form that while one of them applied itself to the upper surface of the leaf when in the fully expanded state, the lower supported the midrib, for the reception of which it was suitably grooved. A figure of this instrument was given in our former paper.† We have now adopted a much better mode of immobilization which will be understood from the figure. Before otherwise interfering with the leaf, plaster of Paris, which has just been mixed with water, is introduced between the lobes at either end of the midrib, so as to bridge the space between them. A splinter of dry wood is laid across from edge to edge of the lobes, the ends of which are cemented by plaster to the marginal hairs. The leaf so prepared is unable to close (figs. 2, 3).

Fig. 2.



The Leaf, seen from above.
The two end plugs and the cross-bar, which stretches from margin to margin, serve to prevent it from closing.

Fig. 3.



The same Leaf, seen from the side. (Both figures are copied from photographs.)

B. *The galvanometer* used is a Thomson of 5244 ohms resistance. The degree of sensibility best adapted for the ordinary purposes of the investigation is such that with a thousandth of a Daniell and a resistance of 10,000 ohms, added to that of the galvanometer, we have a deflection of 350 scale; but for some rheotome observations, a much higher sensibility is required. With a view to more accurate reading of small deflections, a concave mirror, 10 centims. wide, is placed in front of and below the scale, by means of which that part of the graduation nearest to zero can be seen enlarged.

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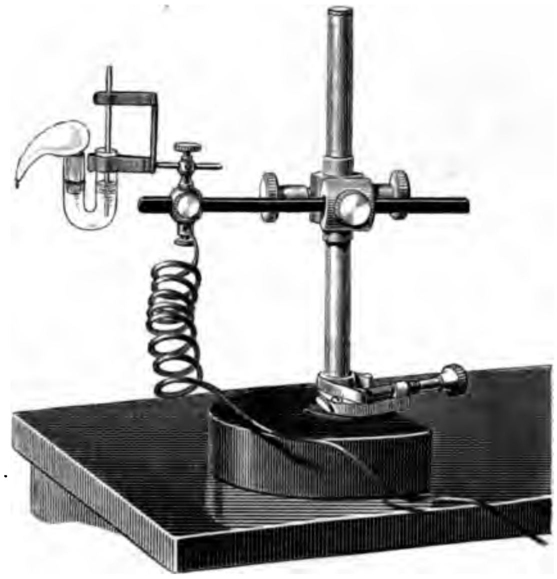
* Vol. i., 1876, p. 24.

† Proceedings, 1873, p. 413.

The electrodes.—The most important part of each electrode is a U-shaped tube containing saturated zinc sulphate, which is supported by a holder of the form shown in the figure (fig. 4). Into the end of the U contained in the clamp, sinks a zinc rod, which is held by the spring clip, and is thus brought into metallic connexion with the coiled wire. This rod is hammered smooth at the end which is intended to be immersed, polished, and amalgamated by treating it sparingly with very weak solution of mercury in nitrohydrochloric acid. Into the opposite limb of the U slips a narrow straight tube 1 centim. in length and about 5 millims. in internal width, which is charged with paste of kaolin and solution of zinc sulphate. The tube has a rim of sealing wax at the top, which prevents it from sinking too far, and is surrounded by a conical beak, shaped with the fingers out of modelling clay (kaolin made into a paste with 0·75 per cent. solution of chloride of sodium). In order to bring the modelling clay to a pencil-like point, a length of several strands of ligature thread steeped in the solution is incorporated with it. These electrodes have a resistance of about 6000 ohms and keep in order for any required time if the atmosphere is saturated or nearly so. The construction of the supports for the electrodes can be understood from the figure. It is sufficient to notice that the collar slides on the horizontal rod of ebonite. This rod can be raised or lowered, or made to rotate round the

Fig. 4.

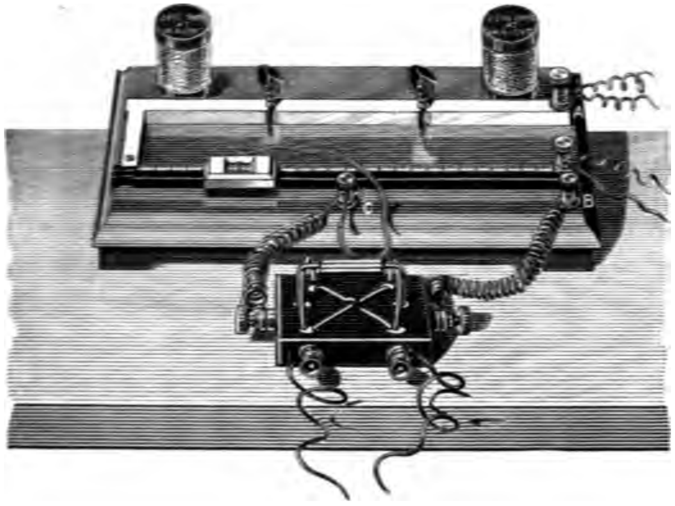


A Non-polarizable Electrode. (From a photograph.)

vertical axis, by working screws, of which the milled heads are conveniently placed for the purpose.

The compensator.—As is well known to every reader of physiological books, Professor DU BOIS-REYMOND has applied the term to an instrument which consists essentially of a long thin wire connecting the poles of a standard cell. The wire being of uniform thickness, the difference of potential between any two points in its length is to the whole difference between the ends, as the distance between these points is to the total length. In a compensator so constructed the total difference has to be determined by a previous operation. The compensator we employ differs in no respect in principle from the potentiometer of Mr. LATIMER CLARKE* in which the total difference is equal to the whole electromotive force of the standard cell, and not, as in DU BOIS' compensator, to a certain portion of it. The form we have given to the instrument is very different from that of Mr. CLARKE'S instrument, and far more convenient for our purpose (fig. 5). The compensator wire, or rheochord, has a total resistance of 5 ohms. On a board, 27 centims. long, is fixed a rail of vulcanite 1 centim. broad and as much high, and 25 centims. long. Along the upper margin of this rail, which is divided into millimeters, runs a wire of platinum, of which the resistance is equal to a ¼ ohm. Each end terminates in a metal block. A second rail or bar of brass runs along the board parallel to the first. It is broken at two points for connexion with the end of the coils 1 and 2, of which the resistances are severally 2¼ and 2½ ohms, so that when the plugs are out the total resistance between the ends of the bar is 4·75 ohms. The bar is united at one end with one of the terminals of the graduated wire. At the other it has a binding screw with two holes. The opposite terminal of the graduated wire has two binding screws, one with two, the

Fig. 5.



Compensator and Reverser. (From a photograph.)

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* "On a Voltaic Standard of Electromotive Force," Proceedings, 1872, p. 444.

other with one hole. On the vulcanite rail runs a brass sliding block or rider, the form of which is seen in fig. 5. It rests on the surface of the graduated wire by its cross wire, and is connected with the binding screw by a flexible wire. From the construction it is obvious that the difference of potential between the binding screws B and C, is to the total difference between A and B, as the length of wire, in millims. between B and the point at which the cross wire rests on the graduated wire is to 5000 millims. In other words, that each millim. of the graduation corresponds to $\frac{1}{5000}$th of the total difference. In use, A and B are connected by one pair of wires with the standard cell through a galvanometer, by the other pair with an "auxiliary battery" through a rheostat as described in Mr. LATIMER CLARKE's paper.

Measurement of electrical resistance.—It is of great importance to have the means of becoming promptly aware of the changes of resistance which occur in the course of a long observation. For this purpose we adopt the plan of from time to time measuring the deflection due to 10 millims. of wire = $\frac{1}{500}$ DANIELL. In investigating plant structures that method is pratically of great value, for the resistances we have to encounter vary, roughly speaking, between 50,000 and 100,000 ohms. As, therefore, we know approximately that our circuit, when the electrodes are in contact, is about 12,000 ohms, viz.: 6000 for the galvanometer and about as much for the electrodes, such changes as often occur are obvious enough. In the mode of leading off used in the fundamental experiment (see Part IV., p. 25) we obtain, if the contact is successfully made, with an over compensation of $\frac{1}{500}$ DANIELL a deflection of about a tenth of the deflection due to the same electromotive force when the electrodes are in contact with each other. If it is less than that, we regard the leading off as unsatisfactory. For actual measurement, we have at hand on an adjoining table a WHEATSTONE's bridge arrangement, which can be brought into connexion with our circuit without loss of time when required, by means of a switch which is substituted for the key which otherwise receives the wires from the leading off electrodes.*

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* This corresponds to K^5 in the diagram. Here L represents the leaf, *f* and *m* the leading off contacts, *x* and *x'* the exciting contacts, I and II the primary and secondary coils, R the rheotome, C the com-

Fig. 6.

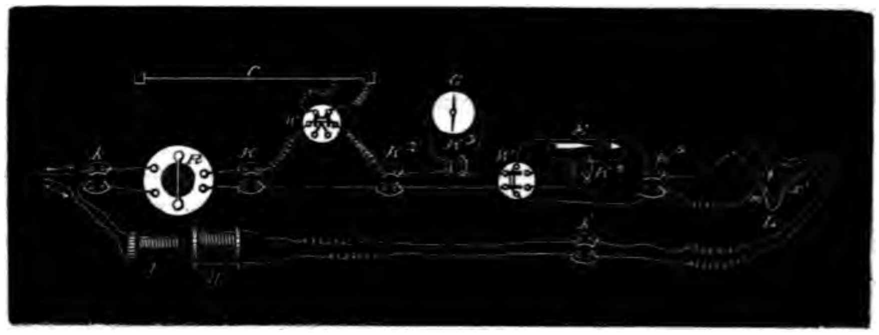


Diagram of Connexions of Compensator.

C. For the preliminary investigation of the time relations of the electromotive changes which follow excitation the most valuable instrument is the capillary electrometer. Two important improvements have been recently made in the construction of this instrument by my friend Professor LOVÉN,* of Stockholm. The first of these consists in constructing the tube which contains the dilute sulphuric acid, of extremely thin glass so that the capillary tube which is in contact with its internal surface can be observed with a magnifying power of at least 300 or 400 diameters (No. 10 HARTNACK). The other consists in cementing the sulphuric acid tube to the mercury tube, so that no evaporation takes place. By the first, the instrument is rendered much more sensitive, by the second, variations in the strength of the sulphuric acid which arise from evaporation are avoided. When used for the purpose of observing a rapid succession of changes in the electrical relations of two led off surfaces, its superiority to the galvanometer is very striking. The movements of the mercurial column not only correspond closely in time to the actual changes which they represent, but express with very great accuracy the differences of potential which actually exist in each successive phase of a variation. In the beginning of an investigation this is invaluable, for the summary information of the duration, character, and progress of the electrical disturbance produced by excitation, which the electrometer affords, renders it possible to avoid loss of time in the subsequent application of more accurate modes of measurement. For the rougher kinds of time measurement, the use of the electrometer may be combined with that of the electro-magnetic signal, in the manner explained in our former paper. By this means the occurrence of an event can be determined within a tenth of a second by a single observation, and, of course, much more accurately by repetition. As a means of measuring electromotive force, the electrometer may be used with great advantage when the electrical states to be investigated are transitory, and at the same time so irregular that the rheotome could not be applied. For this purpose it is much better to measure the electromotive value of each excursion with the aid of the compensator than to deduce it, as has been done by FLEISCHL,† from the "compensation pressure." With this exception the measurements of electromotive force can be made much more accurately with the galvanometer. If it were possible to photograph the motions of the mercurial column of the electrometer it would be an addition to its usefulness.

Use of the Pendulum Myograph as a Rheotome.—The pendulum myograph (fig. 7) is essentially a seconds pendulum, the weight of which carries a glass plate so fixed

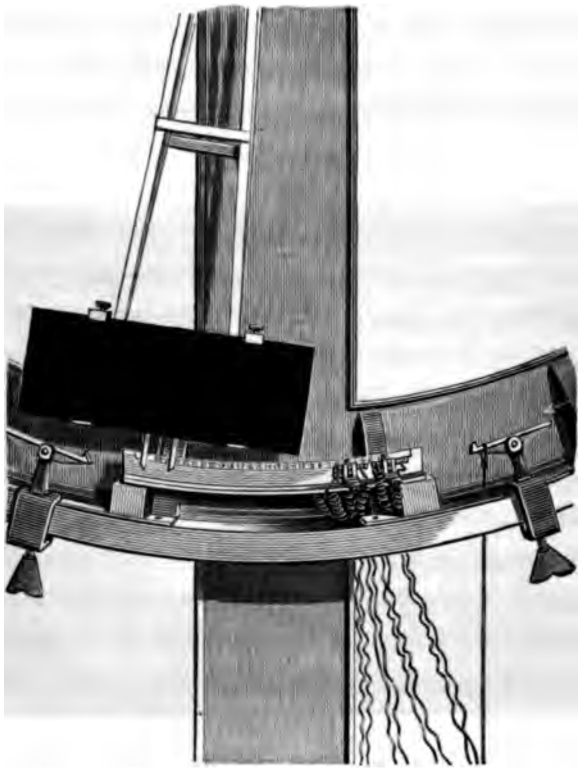
pensator, G the galvanometer, E the capillary electrometer. The wires marked with arrows lead to a battery. K, K^1, &c., are keys. W is a switch by which the wires from the electrodes may be connected either with the galvanometer (as shown in the diagram), or with the electrometer (as when the bridge is turned to the right). *w* is the reverser connected with the compensator shown in fig. 5.

* LOVÉN, "Om kapillarelektrometern och kviksilfertelefonen," Nordiskt medic. Arkiv. vol. xi., No. 14.

† v. FLEISCHL, "Ueber die Construction und Verwendung des Capillar-Electrometers, für electrophysiologische Zwecke." Archiv. f. Anat. u. Physiol., 1879, p. 269.

that the surface oscillates in its own plane. If this surface is blackened, and a tuning fork is allowed to inscribe its vibrations on it during its swing, it is easy to determine the time occupied by the pendulum in accomplishing any given distance taken at any part of its course from the tracing. I have provided my pendulum with a fixed circular scale, on which each division is at such a distance from the next that the trigger in passing from one to the other occupies one hundredth of a second. Against this scale slide four keys, as seen in the drawing. Each key is so constructed that the trigger in passing, opens it. By means of these four keys two circuits can be independently closed and opened one after the other, and the time of closing and of opening can be determined with great exactitude, according to the following plan (fig. 8).

Fig. 7.



The Pendulum-Rheotome (represented as if in motion).

The motion of the pendulum is from right to left. The detent (on the right) is provided with a wire, by means of which the pendulum can be "let off" by the observer at the galvanometer, which must necessarily be at a distance. (From a photograph.)

Provided that the events of which the time relations are to be investigated occur within a third of a second, this is an excellent form of rheotome. In opening a key of the kind employed no time is lost; consequently, the time-interval of closure of the circuit corresponds exactly to the distance between I. and II., or between III.

and IV., as the case may be. When longer periods have to be investigated the rheotome previously described by me, in relation to my experiments on the time relations of the excitatory process in the heart, answers perfectly.

D. *Methods of Excitation.*—For electrical excitation (whether by the voltaic or induction current) it is necessary to use non-polarizable electrodes. As the space between the upper surfaces of the lobes is insufficient to allow of the introduction of two electrodes of the form used for leading off, it is convenient to use a simpler form which, although not so perfect, answers the purpose completely. It is merely a zinc rod of which the amalgamated end is sheathed in wash-leather steeped in solution of zinc sulphate. The sheathed end is enclosed in a larger sheath of the same material, steeped in solution of salt, which ends in a point convenient for contact. The zinc rods are held by supports which allow of their points being brought into any required position with accuracy.

Fig. 8.

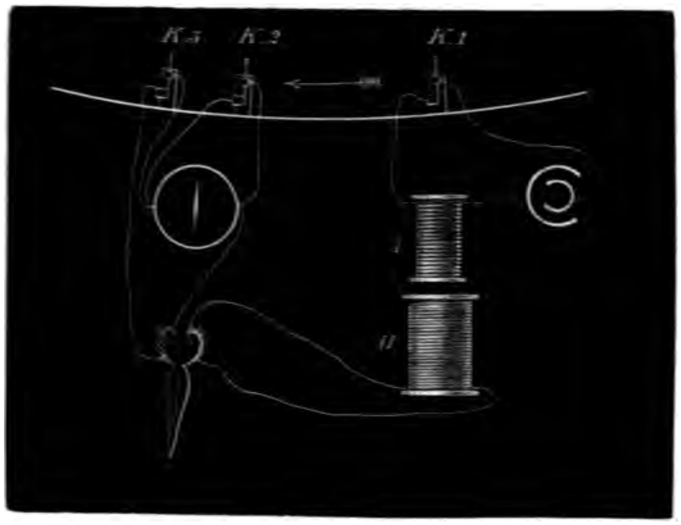


Diagram of the connexions of the Pendulum Rheotome as used for determining time of commencement of first phase, rate of propagation, &c.

PART III.

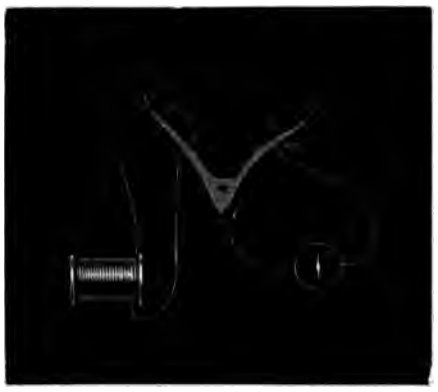
ELECTROMOTIVE PROPERTIES OF THE UNINJURED LEAF.

The preliminary examination which was made in 1876 of the electromotive properties of the uninjured leaf appeared to show, first, that the external surface of each lobe is always positive to the internal surface, and, secondly, that in general the part of the midrib which is nearest to the sensitive hairs is positive to other parts of the external surface of the leaf. The more complete knowledge which has been acquired by work done since, justifies the statement that these two propositions express the truth.

With reference to the second and less important of them, I will refer to the following observations made in the Laboratory of the Royal Gardens at Kew in the summer of 1878.

Negativity of lobe to midrib.—A leaf having been cut off and placed in the warm chamber, of which the temperature varied from 29° C. to 33° C. with the leaf-stalk in water, leading off contacts were made on the external surface of one lobe (*m*) and on the midrib *f* (see fig. 9). The difference* of potential was at first −0·006 D. The leaf was then excited by touching the right central hair at intervals of 5″ for 70′. Each excitation produced a normal diphasic variation, indicating that *m* became first negative, then positive to *f*. After each excitation the difference was compensated. After the first two it fell to −0·0026 D. and thereafter to −0·0006 D. In other words, the after-effect of excitation (see Part V.) was opposed in direction to the original difference. A second leaf was prepared in the same way and left in the chamber, of which the air was nearly saturated, all night, the temperature being 29° C. At 11.40 next morning the difference was −0·004 D., having been −0·009 D. the evening before. After excitation of the hairs of the opposite lobe it fell to −0·0026 D., and subsequently to −0·001 D. A third leaf, similarly led off and placed in a chamber as before, showed a difference of −0·0214 D., the lobes and marginal hairs being fully expanded. After the first mechanical excitation of the hairs of the opposite lobe, the marginal hairs became incurved without any further change of form of the leaf; the difference fell to −0·019 D. After a second excitation it was −0·015 D. and then sank, after repeated touches of the hairs, until, 75 minutes after the first observation, the leaf escaped from the holder without further disturbance of the leading off contacts, the last reading being −0·012 D. It remained

Fig. 9.



Leaf led off from under surface of right lobe and under surface of midrib, excited from left lobe.

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* To save unnecessary repetition in the following pages the word difference, printed in leaded type, will be used for "difference of potential." It need scarcely be added that the letter D. stands for DANIELL.

in the chamber until next morning, when a fine glass rod was introduced between the marginal hairs for the purpose of excitation; the difference was now −0·007 D. The leaf was subjected to a series of excitations, each of which was followed by a normal diphasic variation. A fourth leaf was led off in the same way, but it was not, like the preceding ones, restrained by the glass holder. It was placed in the warm chamber at 1.30 P.M. and observed at intervals until 4.30 P.M. The difference varied from −0·032 D. to −0·029 D. On returning next morning it had diminished to −0·018 D. It was at once excited by gently touching the distal sensitive hair of the led off lobe, the leaf responding by a normal variation but not closing. At once the difference sunk to −0·054 D. After a second excitation of the same kind the leaf closed smartly, the subsequent difference being −0·045 D. Another leaf (not of the same series) was led off by three contacts, one (*f*) at the midrib, the other two at the middle of the external surface of each lobe (*m* and *m′*), so that these surfaces could be severally compared with *f*. Both *m* and *m′* were negative to *f*, the difference being *m* −0·018 D., and *m′* −0·024 D. In another similar observation the corresponding differences were *m* −0·038 D., and *m′* −0·033 D. After a few excitations these diminished to −0·011 D. and −0·004 D.

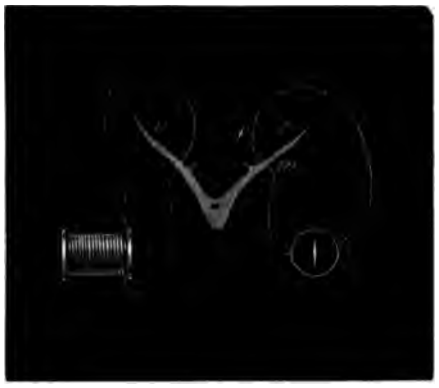
The above observations, with the exception of the two last, were made for the purpose of ascertaining the truth of Professor MUNK's statement, that the external surface of the lobe is negative to the under surface of the midrib. They tend to show that the difference in question is one of frequent (perhaps constant) occurrence in unexcited leaves, and that it diminishes in consequence of excitation.

We now proceed to the study of the second, and much more important, of the two statements made in our previous paper relating to the electromotive properties of the resting leaf. The fact that the external surface of the lobe is positive to the internal surface opposite to it, we propose to designate by the term "cross difference," which sufficiently expresses its own meaning. It will be remembered that the existence of any such difference is denied by Professor MUNK, who holds that the opposite surfaces of the lobe are isoelectrical.

In a preliminary series of observations made in the Laboratory of the Royal Gardens at the same time with those already referred to no exceptions were met with. No importance, however, could be attached to the bare fact of the negativity of the internal surface unless it could be shown to be physiological in its nature—that is, not a result of the merely chemical or physical differences of the two opposite surfaces of contact. The method which is employed for determining whether a phenomenon is physiological or not, must always be the same. It consists in observing whether its development is so associated with the functions of the living part or organism in which it has its seat as to indicate that it is dependent upon it or forms part of it. It was therefore the first step towards determining the physiological meaning of the cross difference, to ascertain whether it is modified by changes in the vital activity of the leaf, and particularly whether it is the same in unexcited and in excited leaves, and whether the change produced by excitation, if any, is in relation with the excitatory process itself.

The first observation on this subject was made in September, 1879, in the course of experiments made for other purposes. A leaf was led off as shown in the diagram (fig. 9)—that is, by opposite contacts, of which the internal corresponded to the space between the distal and proximal sensitive hairs, the surfaces of contact being as large as possible. The leaf was placed in the chamber at 38° C., and was kept there for an hour, at the end of which the two surfaces were nearly equipotential, there having been previously a difference of −0·007 D. During a period of 40 minutes the leaf was subjected to a series of 16 excitations, in the course of which the difference rose from +0·001 D. to +0·0114 D. After a second series of 15 excitations it rose to +0·032 D. In another leaf, observed the following day by the same method, the difference increased during 14 excitations from +0·0084 D. to +0·046 D. In a third the initial difference was +0·0036 D. During the following two hours the leaf was excited 28 times at nearly equal intervals. At the end of the first half hour the difference was +0·0082 D.; at the end of the second, +0·118 D.; of the third, +0·0178 D.; of the fourth, +0·024 D. In the first of these three observations the leaf was excited mechanically—that is, by touching the sensitive hairs of the opposite lobe. In the second and third, single opening induction currents were led through the opposite lobe by pointed electrodes which penetrated its external surface.

Fig. 10.



Leading off electrodes on right lobe. Exciting electrodes on left.

It was not until the next year that the opportunity offered of further investigating this effect. A leaf was led off for the purpose, as in the previous experiment, by opposite contacts, the electrode being applied to the internal surface between the distal and proximal sensitive hairs. To ensure a more satisfactory initial determination of the cross difference in the unexcited leaf, it was introduced into the chamber (of which the temperature was 20° C.) the day before, in order that any accidental difference due to initial changes at the surface of contact might be got rid of. The observations were as follows:—Before excitation the cross difference was +0·024 D.; immediately after excitation, by exciting mechanically the sensitive hairs of the opposite lobe, it was +0·031 D., but soon diminished to +0·026 D. A second excitation had a similar effect—it rose to +0·029 D. After an hour's rest in the chamber the experiment was repeated. Before excitation the cross difference was +0·025 D.; on compensating as quickly as possible it was found to have risen (about half a minute after excitation) to +0·035 D.; in five minutes it had fallen to +0·028 D. A second excitation similarly observed raised the cross difference to +0·031 D., after which it fell to +0·028 D. Four similar observations gave corresponding results, but the effect produced (increase of the cross difference), which in the first experiment of the series amounted to one-hundredth of a DANIELL, did not in the later ones exceed one-five hundredth. The indication that these phenomena afford that the electrical difference which exists in the unexcited leaf of Dionæa between the opposite surfaces is in physiological relation with the excitatory properties of the leaf will receive confirmation when, in the next part of this paper, we have before us the immediate effects of excitation.

In the short communication I made to the Royal Society in 1873, in which I announced the discovery of the excitatory electrical change in the leaf of Dionæa, I stated the existence of a "leaf current" directed from the proximal to the distal end of the midrib in the uninjured leaf. I had observed that, in general, the attached end of the midrib was negative to the further end, and expressed this fact in the language commonly used in physiological writings. I had also found that the under surface of the leaf stalk in the neighbourhood of the joint by which the leaf is attached to it, was negative to the rest of the leaf stalk, and designated this negativity by the term "stalk current." I had further observed that if the current from a battery (a small DANIELL's cell) was directed through the leaf stalk at the same time that the two ends of the midrib were led off to the galvanometer, the difference previously existing between the ends of the midrib was increased if the current led through the leaf stalk were in the same direction with the leaf current, diminished if it were in the opposite direction. I had finally observed that in leaves severed from the plant, the "leaf current" was curiously dependent on the length of the petiole,* so that the shorter the petiole the greater was the negativity of the proximal end as compared with the distal. Of these four observations relating to the electromotive properties of the unexcited leaf, all may be readily demonstrated in most leaves. The more complete knowledge of the subject which I now possess leads me to regard the first two as of little importance. The third possesses a greater interest, for it affords evidence that in the plant a voltaic current conducted through a living part not only influences the condition of the part through which it flows, but extends that influence to extra-polar parts, affecting them in the same way as in nerve, so that whatever may be the nature of the action in the two cases, the phenomena scarcely admit of being distinguished from each other, as may be seen from the following experiments made in the Kew Laboratory in July, 1878.

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* The experiment was related as follows:—"In a leaf with a petiole an inch long I observed a deflection of 40 scale. I then cut off half, then half the remainder, and so on. After these successive amputations, the deflections were respectively 50, 65, 90, 120." (Proceedings, vol. 21, p. 495.)

A leaf was led off at the points *f* and *m* and *f′ m′* in the diagram (fig. 11). The negativity of the attached to the free end of the leaf required 0·01 D. to compensate it, which was expressed by a deflection of 27 scale of the galvanometer. Between the two led off surfaces of the petiole there was a difference of 0·006 D., the leaf end being negative to the other. These measurements having been made, the contacts *f* and *m* were led off to the galvanometer, while *f′* and *m′* were connected with the compensator. The compensator was then reversed so as to conduct a current into the petiole opposite to that which would have been required to compensate it—in the same direction, therefore, as the natural difference—and the slide was pushed along until the needle of the galvanometer was brought to zero. The slide was then at a position indicating +0·024 D.

Fig. 11.

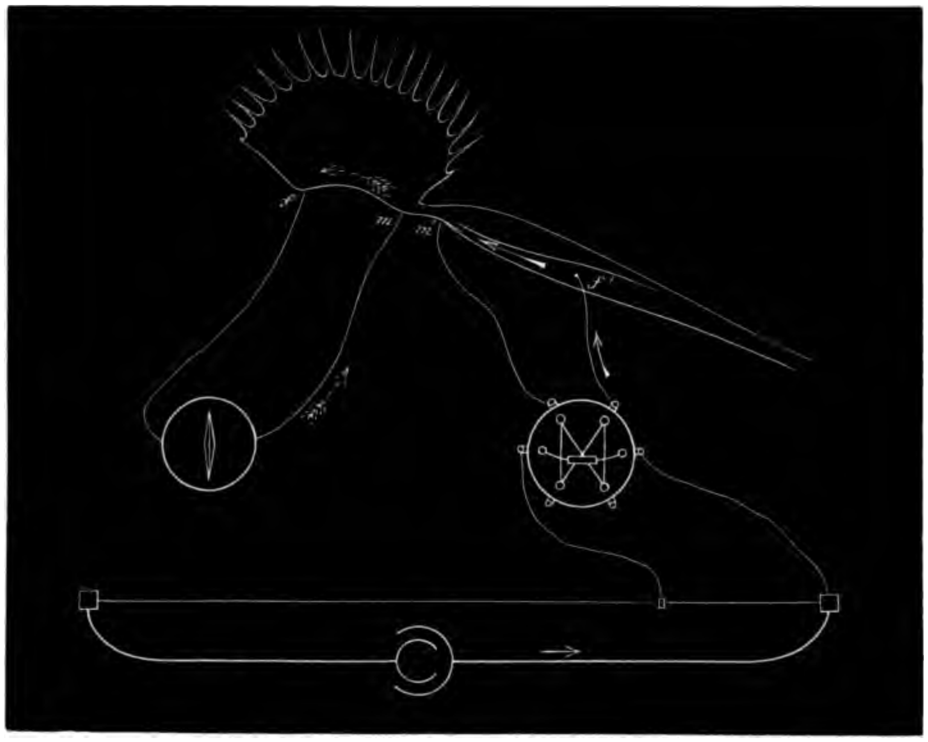

Diagram of experiment as to extra polar-influence of voltaic currents. The strong arrows indicate the direction of the derived current; the weak arrows that of the current determined by it in the leaf. N.B.—The reverser should be turned the other way.

I have since endeavoured to determine the relation between the remarkable extra polar effect (which so strikingly resembles the electrotonic variation of nerve) and the strength of the current which produces it, more accurately. For this purpose currents of various strengths, derived from a rheochord, were conducted through the leaf stalk, the leaf being led off as in the experiment last described. The effects observed were as follows:—With 25 centims. of rheochord wire,* corresponding to a difference of potential of about a quarter of a DANIELL, the difference between the two leaf electrodes was diminished when the derived current was directed from the leaf, from —0·007 to —0·0058, and increased to about —0·008 when it was reversed. With twice the length of wire (about half a DANIELL) it was diminished from —0·006 to —0·004 when the derived current was directed from the leaf. Finally, with three times the length of wire the difference, which had now fallen to —0·005, was abolished; so that when the galvanometer was uncompensated, the needle was brought to zero by the derived current through the petiole.

It was ascertained by careful experiments that the electromotive change in the leaf ceased with the current which induced it. When the experiment was so arranged that the galvanometer circuit having been open during the passage of the "polarizing" current was closed at the moment that it ceased, the needle remained motionless.

In the above experiment it is seen that the induced difference of potential between the leading off contacts was very small indeed as compared with that of the contacts by which the derived current entered and left the petiole. But it is to be borne in mind that this relation does not express the true relation between the electromotive forces, for this reason: We have already assumed that all electrical changes dependent on physiological action, have their seat in the parenchyma, and this assumption is as applicable to the leaf-stalk as to the leaf itself. Consequently the "polarizing" effect of a voltaic current led from one point to another of the surface of the leaf-stalk depends, not on the electrical difference thereby determined between the surfaces of contact, but on the differences between the end cells of the interpolar stratum of parenchyma. Now it is known experimentally, first, that the resistance encountered by a current led as above described through a leaf-stalk is enormous, and secondly, that this resistance has its seat at the surface, whereas the longitudinal resistance of the middle stratum is relatively small. The result is that the physiological or "polarizing" effect of the current on the parenchyma which is the immediate effect of the electrical difference determined by the current between the most and the least positive (or negative, as the case may be) cells of the parenchyma, is a very inconsiderable fraction indeed of what it would be if the polarizing electrodes could be applied directly to the parenchyma itself. These considerations lead me to believe that although in the experiment related on the previous page the currents used were relatively considerable, there is no reason to suppose that the electrical changes in the parenchyma exceeded those (to be described in the next part of this paper) which, in the excitable parenchyma of the leaf are associated with the excitatory state, and consequently I am led to regard it as possible that these changes may not be inadequate to produce secondary changes of the same kind in the petiole—in other words, that if the petiole were led off during the excitatory variation, it would show a variation in the same direction. Want of material has rendered it impossible to settle this important question experimentally.

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* With reference to this mode of using the rheochord, see p. 42 (Excitatory influence of the voltaic current).

The statement contained in my preliminary note in 1873, that the amputation of the petiole always increases and often doubles or trebles the electromotive force of the "leaf current," in other words, increases the negativity or diminishes the positivity of the end of the leaf which is next to it, has been confirmed by further and more exact observations. In illustration of it the following experiment may be given. A leaf was led off in the way described in my note by the two ends of the under surface of the midrib, with the fixed electrode at the distal end, it being still on the plant. The difference between the two surfaces was found to be −0·011 D. On excitation by touching a hair a normal variation was observed, consisting of a positive deflection followed by a prolonged negative after effect, that is, the same phenomena as were described in 1873. On cutting off the leaf with as long a petiole as possible, no effect could be observed on the difference. Half the petiole having been removed without effect, the difference being now −0·0112, half the remainder was removed, the result of which was that the difference increased (the observation being made as soon as compensation could be effected, that is, within a minute after the amputation) to −0·0198. In the course of a few minutes the difference sank to −0·01, that is, a little below its original amount. Amputating half of the remainder of the petiole still attached raised it at once to −0·023, but it soon sank to −0·017. Clipping off the stump of the petiole which still remained, produced only a slight increase to −0·02. Throughout these experiments the excitatory variation exhibited the same character. We have thus before us two sets of facts relating to the electrical influence of the petiole on the leaf, viz.: (1) that a current through the petiole, if directed towards the leaf, renders the attached and proximal end negative to the distal, and (2) that removal of the petiole produces the same effect. I am unable at present to explain either of these phenomena. The nature of the electrical relation between the two organs which underlies both must remain undetermined until it is possible to make further experiments.

PART IV.

THE ELECTRICAL EFFECTS OF EXCITATION.

1. *The fundamental experiment.*—Under this term I propose to designate the observation of the electrical effects which present themselves when the leaf is led off by opposite surfaces of one lobe, while the other is excited by any of the methods

described in Part II. It is to be understood that the electrode *f*[*] is always applied to the internal surface of the leaf between the three hairs (in the trigone) and the electrode *m* to the outside surface exactly opposite *f*. What happens under this condition is always the same. After a delay—of which the duration varies with the temperature—the external surface becomes first negative, then positive, to the internal. For the purpose of giving a general view of the time-relation of the first phase of the variation, I submit the following table, which gives the results of three successive rheotome observations[†] :—

	Time after excitation at which galvanometer circuit opened.							
	0·2"	0·4".	0·6".	0·8".	1"·0.	1"·2.	1"·4.	1"·6.
Obs. I.—Deflection . .	0	3	8	22	28	25	29	16
(100 sc.=0·063 D.)	0	**4**	**10**	**28**	**35**	**31**	**38**	**20**
Obs. II.—Deflection . .	1	19	30	45	44	29	8	2
(100 sc.=0·045 D.)	**1**	**17**	**27**	**40**	**39**	**26**	**7**	**2**
Obs. III.—Deflection .	0	5	36	55	42	17	21	7
(100 sc.=0·0353 D.)	**0**	**3**	**25**	**39**	**30**	**13**	**15**	**5**
Mean result . . .	**0·3**	**8**	**20**	**36**	**35**	**23**	**20**	**9**

Each of the leaves employed in the experiments of which the results are given was fixed in the glass holder described in Part III., and led off, as above explained, by opposite contacts. The three observations were made consecutively; the opposite lobe to the one led off was excited by the rheotome (see fig. 6), the electrodes used being steel needles, of which the points (2 millims. apart) were inserted into the external surface of the lobe. It was consequently necessary to move them three or four times during the period of observation, care being taken that each new insertion should be at the same distance from the middle line of the leaf as its predecessor. Every excitation was repeated twice, and, if the deflections obtained differed in extent, a third time. As it was of much more importance to complete the observation while the leaf remained in a normal condition than to obtain great accuracy in the readings, they were not repeated more frequently than appeared necessary. In each experiment the leaf was introduced into the warm chamber at 32° C. some time before the observation began. The observations were made at intervals of two minutes and a half, and the number of excitations necessary for each was from 20 to 25. Consequently, each lasted nearly an hour.

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[*] In using the letters *f* and *m* to designate the leading off electrodes, it is assumed, as in former papers, for convenience of description and for no other purpose, that any difference or current is to be called negative when it is of such a nature that *m* becomes negative to *f*, and *vice versâ*. The words "negative" and "positive" therefore have only a relative meaning.

[†] The numbers in ordinary figures represent the actual deflection observed; those in black type, what they would have been had the resistance been equal in all the experiments and such that a difference of 0·05 D. would have been expressed by 100 scale of the galvanometer. The sensibility of the galvanometer was purposely reduced for this preliminary experiment, it being desirable to lengthen the periods of closure to 0·2".

The observations given in the table serve to show that the variation lasts during the whole of the first and the greater part of the second second, and that it culminates at from six to eight-tenths of a second after excitation. For these purposes it was thought best to employ long periods of closure (using the galvanometer in a very insensible state) and to make the end of one period coincide with the beginning of the next in such a way as to cover the whole period to be investigated. But such a method was quite inadequate to give information as to the question of greatest importance in relation to the excitatory variation—that of the time at which the effect commences. For this purpose it was necessary to shorten the periods of closure, and consequently to increase the sensibility of the galvanometer. We therefore substituted for the revolving rheotome the "pendulum" rheotome, of which the description is given in Part III. The instrument is so arranged for the experiment that on swinging from right to left it opens three keys in succession—K 1, K 2, K 3 (see fig. 8). Of these, the first occupies a fixed position, so that it is opened about 0·1″ after the pendulum is liberated. This position corresponds to zero of the pendulum scale. The other two keys, K 2 and K 3, can be slid along to any required distance from K 1. The binding screws of K 1 are connected with the primary circuit of the induction coil; those of K 2 with the shunt of the galvanometer, so that when this key is closed the galvanometer is "short circuited;" those of K 3 are interpolated in the galvanometer circuit between the leading-off electrodes and the shunt. Consequently, when the three keys (being previously closed) are opened in succession by the pendulum, the primary circuit is first opened by K 1, the galvanometer circuit is unbridged—that is, closed—by K 2, and again broken by K 3. As the scale of the pendulum is divided into hundredths of a second and extends over a period of a quarter of a second, this arrangement affords the means of investigating with great exactitude the events of the beginning of the variation.

The galvanometer is brought to such a degree of sensibility that with a resistance of 10,000 ohms, in addition to that of the galvanometer in circuit, 0·0001 D. gives a deflection of 97 scale. To facilitate accurate reading a slightly concave mirror is placed in front of the scale, by means of which an enlarged image of the part of the scale most used can be seen by the observer without fatigue. The leaf is led off and excited as in the fundamental experiment. For excitation, non-polarizable electrodes of the form described at D, p. 18, are used, the contacts being opposite, and corresponding in position to those of the leading off electrodes applied to the opposite lobe. The plant of which it forms part is placed in the chamber, the air of which is saturated, at 26° C.

It having been first ascertained that when K 2 was at 0·2″ of the scale of the pendulum, and K 3 at 0·04″, there was no variation, successive observations were taken with K 3 at positions further and further removed from K 2 (which remained stationary at 0·02″) with the following results :—With K 3 at 0·06″ there was a trace of movement of the needle; with K 3 at 0·08″ it had increased to −4 scale; at 0·1″ to 12; at 0·15″ to −39 sc. To complete the observation K 2 was moved to 0·1″ and K 3 to 0·2″, so that the period of closure now corresponded to the second tenth of a second after excitation; the galvanometer reading was −161 sc.; finally, between 0·2″ and 0·3″ we had −248 sc.

In four other experiments the results (stated shortly) were as follows :—

Experiment I. Temperature 32–35° C. 0·02″–0·4″, no deflection; 0·02″–0·05″, a trace; 0·02″–0·06″, −5 sc.; 0·02″–0·1″, −35 sc., −31 sc.

Experiment II. Temperature 34° C. 0·02″–0·04″, no deflection; 0·02″–0·05″, −3 sc.; 0·02″–0·06″, −6 sc.; 0·02″–0·15″, −70 sc.

Experiment III. Temperature 32° C. 0·02″–0·06″, no deflection; 0·02″–0·08″, no deflection; 0·02″–0·10″, −13 sc., 22 sc., 23 sc.; 0·02″–0·9″, −10 sc., 7 sc., after which previous negative results were verified by repetition.

Experiment IV. Temperature 36° C. 0·02″–0·04″, trace; 0·02″–0·05″, −4 sc., 6 sc.; 0·02″–0·6″, −8 sc., 10 sc.; 0·02″–0·8″, −12 sc.; 0·02″–0·1″, −16 sc.

The experiments were made consecutively, that is, on successive days, and, with the exception of Experiment IV., under precisely similar conditions. In Experiment IV. the temperature was inadvertently allowed to rise to 36°,[*] a fact which sufficiently accounts for the somewhat earlier beginning of the variation.

The experimental results which have been given justify the statement (1) that the first phase of the variation is entirely negative; (2) that it begins (at the temperature of 32° C.) about one-twentieth of a second after the excitation; (3) that its electromotive force rises rapidly at first and subsequently very gradually; (4) that it culminates about the end of the first half-second after excitation and subsides gradually towards the end of the third half-second.

In the paper which I communicated to the Royal Society with Mr. PAGE in 1876, I stated that the variation of the leaf did not begin until two-tenths of a second after excitation, and gave the grounds on which the statement was based. The experiments which led us to conclude that what we then called the "electrical delay" (the interval of apparent electrical inactivity) were correctly made. We repeated them under the most favourable circumstances in the Kew Laboratory in 1878,[†] with perfectly similar results, but nevertheless our interpretation of them was erroneous.

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[*] It is probably also worth noting that the day on which this experiment was made was the hottest of last summer, Friday, July 16.

[†] The observations made at Kew, in 1878, gave 0·12″ as the shortest period of delay when the

In 1876 we used an arrangement by which (1) the act of excitation (if mechanical, the depressing of a lever which brought a camel-hair pencil into contact with one of the sensitive hairs; if electrical, the breaking of the primary circuit of the induction apparatus) recorded itself on a blackened cylinder revolving at a rapid rate by clockwork; and (2) the record of the response was inscribed on the same cylinder.

In the first record, the inscription of which was effected with the aid of DEPREZ' electro-magnetic "signal," or chronograph, the loss of time was inappreciable. The response was the motion of the mercurial column of the capillary electrometer. This was observed by one of us (Mr. PAGE), who recorded the event on the cylinder by closing the current through the "signal" which the other had broken in the act of excitation. The interval between break and make of the signal current comprised: (1) the physiological delay to be measured; (2) the instrumental delay (that is the time lost, if any, in moving the mercurial column); and (3) the "personal time" or "reaction time" of the observer. The last we had no difficulty in eliminating, for Mr. PAGE's personal time was subject to very slight variation. The second we had not taken into account at all, assuming it to be non-existent. The consideration that excitatory difference in Dionæa probably begins gradually, and that if so the motion of the column is slow, and may therefore not be perceived until a longer period than the normal time after it has actually begun, led us later to conclude that our method was inadequate for the purpose, and that the only way by which the question could be settled was by the use of the rheotome and galvanometer.

The after effect.—The change which has been above described as occupying the greater part of the first 2″ after excitation is followed by a period during which the surfaces are nearly, or absolutely, in the same condition electrically as they were before excitation, but sooner or later a secondary effect begins, which I propose to call the *after effect.* During the after effect the internal surface becomes negative to the external.

In order to bring the after effect into view, the rate of the rheotome must be reduced and the periods of closure much lengthened. In our first observation the rate of revolution was 1 in 78″, and the closing time 1·4″. The leaf was excited as before at intervals of three minutes, without being placed in the chamber, the temperature of the room being 18° C., the mode of excitation being the same as in the previous series. After the first excitation the galvanometer circuit was closed between 1·3″ after excitation, and 2·6″. In this case the deflection was negative, for, in consequence of the relatively low temperature at which the observation was taken, the first phase of the

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measurement was made in the moist chamber at 32° C.—a result which exceeds the truth by about 0·05″. Assuming that the error is to be set down, not of course to loss of time in transmission of the effect to the mercurial column, but to the lengthening of the "reaction time" when the motion to be perceived is gradual, it renders all the observations made by signalling with the electrometer useless for the purpose of measuring the duration of the delay, but does not interfere with the use of this method for other purposes, particularly for determining which of the two similar electrical responses occurs earliest.

variation was prolonged so as probably to cover a great part of the closing time. In the second experiment, the closing time being 1·3″ later, namely, from 2·6″ to 3·9″ after excitation, there was a positive deflection which was not measured. In the third, 1·3″ later, the positive deflection was 90, in the following ones, successively, +71, +70, +50, +45, +30, +30, and so on. On placing the leaf in the chamber and repeating the observation a perfectly similar set of results was obtained. Between 0·6″ and 1·95″ after excitation there was a negative deflection of −380. Between 1·3″ and 2·6″ a much smaller negative of −70, these being the expression of the first phase. Then followed between 2·6″ and 3·9″ a positive deflection of +6, after which, at successive consecutive periods, +100, +75, +70, +42, +20, and so on.

From these and other similar observations made by the same method, it appears that in the fundamental experiment the external surface which, 4″ or 5″ after excitation, attains its greatest positivity as compared with the internal, returns very slowly (if at all) to its previous state. Various other methods were used for the purpose of observing the progress of the after effect with more completeness, of which the best is exemplified in the following experiment.

The method employed was as follows :—It having been ascertained that the electromotive force of the after effect at 2″ after excitation corresponded to a deflection of 160 galvanometer scale, of which the equivalent was 0·016 D., the galvanometer having been over compensated to that extent was closed at 1″ after excitation. In this way the needle was brought so nearly to rest that it followed the changes of difference without oscillation, and could without difficulty be read at 5″ intervals. The readings were :—

At 3″,	at 8″,	at 13″,	at 18″,	at 23″,	at 28″,
+9	−50	−130	−142	−149	−160.

That is, at 3″ after excitation the positivity of the external surface was a little more than 0·016, at 8″ less, at 13″ much less, and so on, so that the result in electromotive force might be expressed as follows: +0·017 D., +0·011 D., +0·003 D., +0·002 D., +0·001 D. Accordingly, in another leaf, after carefully compensating the existing difference and breaking the galvanometer circuit as before, we at once adjusted the compensator so that on closing there should be a negative difference of 0·01 D. We then excited, and 2″ after closed the galvanometer circuit. Again the needle followed the slow electrical change, its positions at 5″ intervals being as follows: +70, 0, −60, −120, −140, −150, −150, −160, &c. As the value of 0·01 D. with the preparation in circuit was 190 scale, the internal surface was still 30 scale (= about 0·0015 D.) negative. Repeating the same experiment with an over compensation of 0·015 D. the successive positions of the image were: +20, −40, −140, −200, −220, −240, −250, −260, &c. So that the observation indicated, as before, that in this instance the negativity of the internal surface exceeded the compensation used.

In a third leaf observed at the same time, the resistance being unusually low, it was impossible to get the after effect on the screen, although the difference was not larger than in the other two. On interpolating as before a length of wire corresponding to 0·009 D., and closing the circuit 2″ after excitation, the successive readings at 5″ intervals were: +200, + off scale, +230, +60, −50, −130, −210, −260, −298, −320, −345, −350, −360, &c., the reading due to over compensation being −370; so that here, as in the other instances, the maximum difference was between 0·01 D. and 0·02 D.

Influence of repeated excitations on the intensity and duration of the after effect.— Whenever a freshly prepared leaf is observed with the capillary electrometer during the course of a succession of excitations, it is found that in the first two or three the negative movement of the mercurial column is followed by a positive one which equals or may even exceed itself in extent. As the excitations are repeated, the positive phase becomes smaller and smaller, until after eight or ten, it may very readily escape observation.

For the purpose of obtaining more exact information on this subject, the galvanometer and rheotome were used; for it was obvious that if in a succession of experiments the electrical condition of the led off surfaces could be compared, during a period after excitation corresponding to the maximum of the after effect, that is, the period during which the internal surface of the leaf is most negative, the successive deflections recorded would give a good indication of any such diminution as the observations with the electrometer indicated.

The period of closure chosen was between 3″ and 4·5″ after excitation. The deflections were, of course, all positive, but varied in extent from 175 to 21. In 21 successive excitations the mean of the first seven deflections was 157, of the second seven, 86, and of the third, 55·5. In this experiment the leaf was excited at intervals of not less than two minutes. If the excitations are repeated as often as every half minute it is found that the after effect completely disappears. This can be easily shown by the electrometer, but can be more exactly determined by the galvanometer. The best way to do this is, first to make with the galvanometer an observation of the kind described in the last paragraph (p. 30), then to subject the leaf to a succession of excitations at short intervals, and to follow these by a second galvanometer observation for comparison. The following results are obtained:—

(1.) The fourth excitation of a leaf showed an after effect of which the course was as follows, as indicated by the negative deflections observed at 5″ intervals: +300, +262, +155, +100, +74, +66, +59, &c.

(2.) After ten effectual excitations at half minute intervals the after effect was: +160, +154, +70, +49, +40, +30, +12, &c.

(3.) After ten more effectual excitations it was: +25, +5, 0, 0, &c. The leaf was then left to itself for 15 minutes and the after effect again observed. It was: +85, +130, +62, +52, +49, +40, +36, &c.; and after ten minutes' further rest: +100, +200, +104, +70, +62, +52, +47, &c.

On repeating the excitations at half minute intervals, the after effect was again entirely abolished, reappearing again after a period of rest. It was further found, in another experiment, that if, after a succession of excitations at half minute intervals, a leaf was excited twice at longer ones, *e.g.*, two minutes and a half, the after effect had, by the time the second was made, already reappeared, or if previously diminished had much increased.

In Part III. it was shown that the influence of an excitation in increasing the previous difference (that is, the positivity of the external surface) or in other words, in rendering the external surface permanently more positive than it was before excitation, depended on the interval of time which had elapsed since any previous excitation. In a leaf which has been repeatedly excited at frequent intervals, excitation leaves behind no change in the electrical relation of the opposed surfaces, because there is no after effect. In other words, the persistent change of the potential, consequent on excitation, described in Part III., is a small remainder of the after effect, as it seldom amounts to more than 0·002 D, whereas the after effect itself often attains at its maximum an electromotive force of 0·02 D. It may be roughly stated that the latter is apt to leave behind a remainder of something like one-tenth of its amount. All that can be insisted on as resulting from observation is, that the subsidence of the after effect is extremely slow and incomplete, and that whenever it exists it leaves behind it a remainder—the excitatory increase of positivity of the external surface, already described.

The excitatory process as observed when the leaf is led off by symmetrical (identical) *surfaces of contact of opposite lobes.*—The excitatory variation which is observed in a leaf led off symmetrically, that is, by corresponding surfaces on opposite lobes, is the combined result of equal and opposite electromotive changes which have their seat in the tissues between the contacts. Consequently, if in an unsevered leaf so led off we suppose an excitation to take place at the same moment from each electrode, no variation would be observed, for the electrical changes due to the one excitation would exactly balance those due to the other. The appearance of a variation would indicate inequality of action between the two lobes. If the excitatory response of the right lobe were either stronger than that of the left, or were accomplished in a shorter time, the variation would express the difference. In the former case the right would have the advantage throughout; in the latter, the right would have the advantage at first, afterwards the left.

The case in which two lobes are led off and excited simultaneously and symmetrically has not been realised experimentally. The consideration of it, however, serves to facilitate the understanding of the case which actually presents itself in which the leaf is led off by symmetrically placed contacts on opposite sides, but excited on one side only. In this case experiment shows that there is always a galvanometric effect, but that the character of the variation observed differs in different leaves very considerably—so much so indeed that at first sight the differences appear irreconcilable. Before giving an account of them it will be useful to consider what we should expect. This depends mainly on the question whether, in the transmission of the excitatory effect from the seat of excitation to the opposite lobe, time is lost or not. In the fundamental experiment we have evidence that the excitatory effect travels. For when we excite the left lobe and the right responds, some time is without doubt occupied in transmission. If, as Professor MUNK believes, the transmission is so extremely rapid that the excitatory change may be said to begin at the same moment at all parts, the case would resolve itself into the hypothetical one from which we started, viz.: that of excitation taking its rise at the same moment from both leading off contacts; and this being so, any galvanometric effect observed must be due to one of the two causes referred to above, namely, either to unequal intensity or to unequal duration of the electrical response of the two sides of the leaf. If, in addition to these inequalities, we have to consider the influence of propagation (*i.e.*, if the time lost in propagation is sufficiently considerable to tell), the interpretation of the galvanometric effect observed in an experiment in the form represented in fig. 13 becomes extremely complicated; for the modifications severally due to loss of time and to difference of intensity would interfere with each other in a way very difficult to estimate. It will be shown in a subsequent section that the rate of propagation in the excitatory disturbance is great, as compared with the duration of the excitatory effect; in other words, that its wave-length is considerable. This being so, the question whether or not the modification due to loss of time will show itself, will depend on the abruptness with which the disturbance commences, as will be seen from the following diagrams. If, for example, we suppose that the excitatory electromotive changes are so directed that the led off surface on either side becomes negative to the midrib, and that the rise of negativity on the right side is represented in two different leaves by the curve *a*, that of the left side by the curve *b*, the galvanometric effect during the period represented will be expressed in each figure by the shaded area, the depth of which is greater in the first case than in the second; so that if (as is represented in fig. 12, C) it were further diminished by a greater abruptness in the rise of negativity on the left side, it might escape observation. And it is easy to see that if, in addition to beginning more abruptly the excitatory disturbance on the left side were more intense, the influence of the loss of time in propagation would lose itself entirely in that of inequality. The two last suppositions (fig. 12, B and C) appear to afford the key to the understanding of what is actually observed when a leaf is led off by symmetrical points and excited near one of them. In this case there is always an excitatory effect, proving that the two lobes do not respond equally. Further, we have found that so far from its being true, as asserted by Professor MUNK, that it makes no difference what spot is chosen as the seat of excitation, it never happens that variations of the same character are obtained when, in two successive experiments, or in two series of experiments, the right and left lobe are alternately excited. At first sight it seemed as if this difference were referable to propagation, but more careful consideration has led to the conclusion that inequality of response has a much larger share in its production than loss of time. Ground for this conclusion is to be sought, as regards each particular case in which a symmetrically led off leaf acts unsymmetrically, in the observation that each of its two lobes when separately and successively tested shows the same inequality. Unfortunately, the nature of the case did not present itself in its true relations until too late to do this in a sufficient number of cases, but I have abundant evidence that in general the electrical reactions of the two lobes of a leaf are unequal.

Fig. 12.

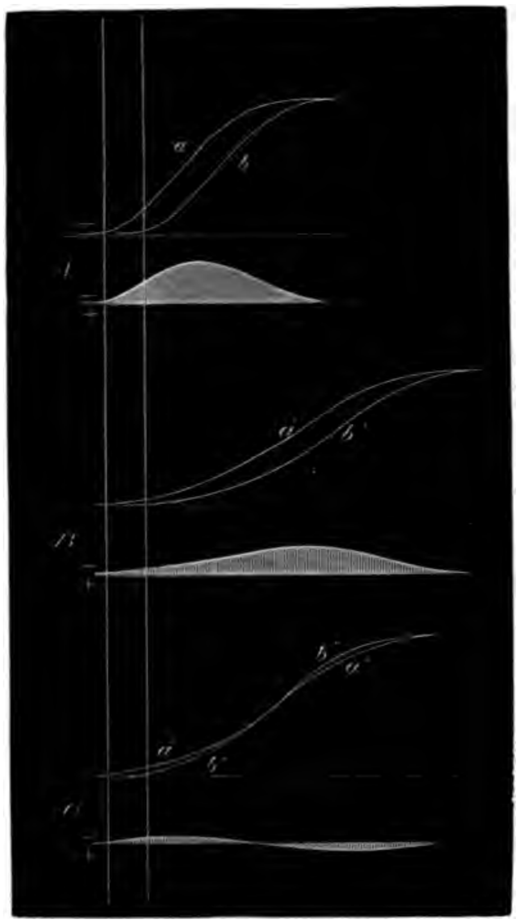

Diagram intended to illustrate the relations of the first phase of the excitatory variation when the leaf is led off by symmetrical contacts on the under surface.

In observations on symmetrically led off leaves the electrodes may be applied either to the internal or the external surface.

Symmetrical contacts on the internal surface of the leaf between the two marginal sensitive hairs.—In order to determine, if possible, the general character of the variation in this form of experiment, four observations were made in succession with the electrometer. The leaves used were led off by symmetrical contacts between marginal hairs of opposite lobes, and subjected, first on one side and then on the other, in each experiment, to two series, each consisting of ten successive excitations. The table shows the results. The letter *f* or *m* indicates that, in the case to which it relates, the fixed or movable electrode was on the excited lobe. In all the experiments the excitation was effected by touching a sensitive hair with a camel-hair pencil. The hair touched is indicated. After each observation the difference was compensated. The mean differences are given in the last column. All of the leaves were tested by observing the excitatory effect in each lobe separately when led off as in the fundamental experiment.

No. of experiment.	Which lobe excited.	Which hair touched.	Which electrode on excited lobe.	Character of variation.	Difference.
I.	R.	Proximal .	M.	Monophasic, −39·1	+0·011 D.
	L.	Ditto .	F.	Diphasic, +11·7, −21·2 . . .	+0·007 D.
II.	L.	Proximal .	F.	Monophasic, +6·2	−0·01 D.
	R.	Ditto .	M.	Diphasic, −2, +25·4	−0·02 D.
III.	R.	Proximal .	M.	Diphasic, −3·2, +18·8. . . .	−0·007 D.
	L.	Ditto .	F.	Monophasic, +22·2	−0·005 D.
IV.	L.	Distal . .	F.	Diphasic, +3·4 pause . . . . +7·4 (from zero)[*] . .	+0·023 D.
	R.	Ditto . .	M.	Diphasic, +9·8, −2·9	+0·023 D.

In leaves I. and II. the difference between the two lobes was so great that the leaves could scarcely be considered normal. In leaf III. the variation was normal on both sides, but the first phase was larger on the left than on the right (−8·5, +4·3 on the left, −4·4, +7·8 on the right). In leaf IV. the first phase was nearly equal on both sides (−30·0 on the left, −31·6 on the right), and was followed in both cases by a normal after effect. But these facts afford no clue to the understanding of the results recorded in the table, excepting in so far as they show that in a well-balanced leaf—*i.e.*, one which responds to excitation with some degree of equality—whichever lobe was excited directly had the advantage over the other in the compound variation. That this was so was confirmed by the observation that in leaf IV. the negative phase of the variation, as observed with opposite contacts (the fundamental experiment), was much smaller (−18·4 on the left, −26·6 on the right) when the opposite lobe was excited than when a hair adjacent to the leading off electrode was touched. The only general conclusion that I venture to draw from the experiments is a negative one, namely, first, that the differences which are observed between the excitatory effects in the two cases are as little due to mere inequality of response in the two lobes as to propagation; for such inequality would not account for the effect produced by changing the seat of excitation. We must suppose that, independently of any difference in physiological activity which may exist between the lobes, the electrical disturbance is more intense in those parts of the leaf which are nearest to the seat of excitation than in those which are more distant from it. This being admitted, and account being taken of the complicated arrangement of the electromotive structures concerned, the incongruity of the results need no longer surprise us.

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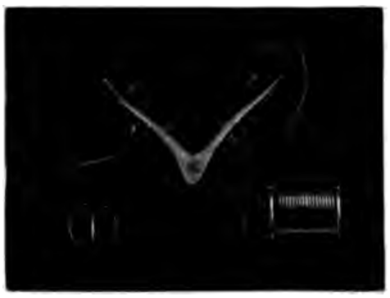
[*] To be read thus:—In the first phase, the meniscus moved from zero of the scale to −3·4, in the second, after a pause, from 3·4 to 7·4 in the same direction.

Symmetrical contacts on the under surface, opposite the space between the sensitive hairs (fig. 13).—The same considerations apply to this case as to that in which the leaf is led off from the upper surface; but inasmuch as the under surface presents no appreciable difference of structure, whereas in the upper we have to do with the sensitive hairs, the prospect of obtaining congruous results is not so hopeless. It will be sufficient to refer to one or two experiments made during the autumn of 1880, in which the galvanometer and rheotome were used for comparison of the excitatory effects.

Fig. 13.



Leaf led off by symmetrical contacts on under surface of lobes.

The first leaf used for the purpose was one in which symmetrical results could hardly be expected. It was a large well-grown leaf which contained the remains of a digested fly; and when, after having been excited an equal number of times on either side, it was allowed to close, the incurvature of the left lobe was much greater than that of the right. This leaf was excited electrically (the exciting contacts, x, being on the internal surface) and led off symmetrically, the electrodes touching the external surface exactly opposite the trigone on either side, the fixed electrode being on the right side. The rheotome gave the following result, the period of closure being 0·2″:—

	x On left lobe.	x On right lobe.
	M.	F.
At 0·4″	−37·5	+50
At 0·6″	− 9·0	+41
At 0·8″	+33·0	+40

The numbers given are the means of two accordant observations at each period, before each of which the difference was compensated.

In a second leaf, led off in precisely the same way, the general character of the excitatory effect was observed with the electrometer. The fixed electrode was on the left lobe. When that lobe was excited electrically (as in the last experiment) the variation was diphasic, the first phase being positive. When the right lobe was excited (*x* on right lobe), the variation was also diphasic, with opposite signs. The rheotome gave the following, the closing time being 0·075″:—

	x On left lobe.	*x* On right lobe.
	F.	M.
At 0·10″	+ 9	− 7
At 0·20″	+24	−18
At 0·30″	+27	−20
At 0·40″	+20	− 9
At 0·50″	+ 4	− 4

The numbers express in each instance the means of two observations, before each of which, as usual, the difference was compensated. The resistance exceeded 160,000 ohms, notwithstanding that the utmost pains were taken in making the leading off contacts.

In a third experiment we had at our disposal a large vigorous leaf. The fixed contact was on the right lobe. The leaf was subjected to 48 excitations during a period of 70 seconds, to all of which it responded. Its resistance was such that 100 scale of the galvanometer corresponded to 0·45 D. The excitations gave with the rheotome (the duration of closure of the galvanometric circuit being one-fifth of a second) the following results:—

	x On right lobe.	*x* On left lobe.
	F.	M.
At 0·20″	+28	−33·5
At 0·4″	+44	−24·3
At 0·6″	+50	− 3·0
At 1·0″	0	0

These observations show, even more strikingly than the previous ones, how completely Professor MUNK is mistaken in his statement that, provided the leading off is the same, the seat of excitation is a matter of indifference. They also serve to strengthen the conclusion that the intensity of the electrical disturbance is greater in the neighbourhood of the seat of excitation than at a distance from it. No other explanation would account for the results.

Rate of propagation of the excitatory disturbance.—The only certain method of determining the rate of transmission of the excitatory change in Dionæa consists in measuring the interval of time after excitation at which the electrical effect first becomes appreciable, by a series of observations in which a leaf, led off as in the fundamental experiment, is excited alternately in the neighbourhood of the leading off electrodes, and at a corresponding part of the opposite lobe. In such an experiment the interval of time between excitation and effect in the one case will be greater than in the other by a fraction of a second. That fraction, multiplied by the distance in millims., will give the rate per second in millims.

In our former paper we attempted to arrive at a conclusion on the subject by a method which we now know to be inapplicable. I will first refer to certain experiments which were made on this principle in the Kew Laboratory in 1878, for the purpose of confirming the statements contained in our former paper (1876). A plant was placed in the chamber at 30° C., and led off as above stated. The excitatory variation on the electrometer scale was -12 followed by $+10$. The leaf was excited by touching the sensitive hairs, alternately on opposite sides, the time of excitation and that of response being determined by the method referred to in Part II. During the period of observation the temperature rose to 37° C. The observations were made in four series, in which the means of the time-intervals between excitation and response were as follows :—

1.	Excitation of the hairs of the	same side	0·08″
2.	„ „ „ „	opposite side	0·28″
3.	„ „ „ „	same side	0·116″
4.	„ „ „ „	opposite side	0·243″

In another leaf the experiment was made first at the temperature of the room, afterwards at that of the chamber 32° C. Six observations gave :—

1.	Excitation of the hairs of the	same side	temp. 18° . .	0·10″
2.	„ „ „ „	opposite side	„ 18° . .	0·27″
3.	„ „ „ „	same side	„ 32° . .	0·05″
4.	„ „ „ „	opposite side	„ 32° . .	0·12″
5.	„ „ „ „	same side	„ 18° . .	0·43″
6.	„ „ „ „	opposite side	„ 18° . .	0·78″

Here the excitation was by induction shocks, the external surface being punctured by fine electrodes for the purpose. This observation shows the combined effect of temperature and local diminution of excitability. The influence of the latter is still more strikingly shown in the following series, the temperature throughout being 18° C.

Leaf fresh :—	
1. Excitation of the same side	0·15″
2. „ „ opposite side	0·26″
After repeated excitation :—	
3. Excitation of the same side	0·50′
4. „ „ opposite side	0·84″
Later :—	
5. Excitation of the same side	0·53″
6. „ „ opposite side	0·78″
Still later :—	
7. Excitation of the same side	1·54″
8. „ „ opposite side	3·50″

These Kew experiments showed that at the ordinary temperature of summer the interval of time between excitation and response is considerable, and that its length depends on the distance of the seat of excitation from the led off surfaces, and is much longer in injured leaves than in fresh ones. But for reasons which I have already stated in discussing the fundamental experiment, it is certain that time measurements made by the method of signalling are incapable of giving an answer to the question of the rate at which the excitatory effect is propagated. During the hot weather of last summer (July, 1881) we had the opportunity of making experiments with the pendulum rheotome which proved in the clearest way that, under favourable conditions, the rate of propagation in the leaf of Dionæa is extremely rapid.

From the extreme difficulty which was met with last summer in obtaining plants in vigorous condition, I was prevented from making as many observations as I deemed desirable. Of those which were made I think it best to submit the results of one only, which was perfectly satisfactory :—

Period of closure.	Same lobe excited.	Opposite lobe excited.
0·02″ to 0·04″*	0	0
0·02″ „ 0·06″	−11	0
0·02″ „ 0·08″	−22	0
0·02″ „ 0·10″	−31	0
0·02″ „ 0·125″	..	9
0·02″ „ 0·15″	..	26
0·10″ „ 0·20″	..	50

In this experiment the plant was placed in the chamber at 38° C. and remained at that temperature throughout. The leaf was led off as in the fundamental experiment. The excitation was by induction shocks. The exciting electrodes were applied on the led off lobe close to the distal and proximal hairs respectively, so that a straight line connecting the contacts would pass through both of them. On the opposite lobe they were applied opposite to each other in the space between the hairs. The observations with excitation on the opposite side were made first, so that the effect of "exhaustion," if any, in increasing the delay, would tell against the result. Two observations were taken at each period, which coincided in most cases exactly. The weather was extremely warm and sunny. In other leaves the time interval between the two sets of results never exceeded that observed in the one recorded, so that I think I am justified in saying that the rate of transmission is at least 200 millims. per second. But it must be carefully borne in mind that this only applies to plants at hot-house temperature, as was strikingly shown in all of the experiments. A leaf, which in the chamber at 32° C. responded to an excitation of the opposite lobe when the galvanic circuit was opened at 0·05″ after excitation, gave no trace of a variation with a closing time which lasted to 0·20″, when the experiment was repeated as soon as possible afterwards in the room at 21°. On shifting K 2 to 0·20″, so that the galvanometer circuit remained closed from one-fifth of a second after excitation onwards, the image shot off the screen.

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* Trace at 0·05.

PART V.

EXCITABILITY.

The investigation of excitability in Dionæa is a matter of great difficulty, partly on account of the rapid changes which it undergoes, partly on account of the difficulty of so applying any excitant that its exciting action can be assumed to be constant. The results of our observations as to the behaviour of the leaf when excited will be best stated in relation to the different methods of excitation.

1. *Mechanical excitation.*—It was observed by Mr. DARWIN ('Insectivorous Plants,' p. 289) that when a single human hair was fixed in a handle, and cut off so that one inch projected, and "the extremity of the hair was then brought by a slow movement laterally into contact with the tips of a filament, the leaf" excited "instantly closed," or, "after two or more touches of the same kind;" whence he concluded that they were extremely "sensitive to a momentary touch" which was best adapted to enable the trap to close on an insect. To this I have to add another observation, which is at first sight so improbable that I have become convinced of its truth with great reluctance, viz.: that the touch is much more efficacious when it is a living touch, that is, when communicated by an instrument (particularly a hair or a camel-hair pencil) held in the hand, of which the motion is muscular, than by any mechanical arrangement which I have been able to contrive. The two forms of mechanical excitation which have been used are (1) a lever, of which the axis works in a firm support of which the height can be varied at will, at the end of which a camel-hair pencil is fixed of which

the point is downwards, and so constructed that when it is depressed by the finger the signalling circuit is closed and the tip of the camel-hair pencil brought into contact at the same moment; and (2) a similar lever of which the motion is due to the action of an electro-magnet included in the signalling circuit. Whichever of these forms was employed, and whatever care was taken in setting the pencil so as to ensure its hitting the hair to be excited without fail, it was invariably found that after a few excitations no further response was obtained; whereas it is always easy to repeat an experiment which I made with Dr. FRANCIS DARWIN in 1873, in which we succeeded in exciting a leaf during an hour at two minutes' intervals, without a single failure of the electrical response. This may, I think, be due to the relative rudeness (if such a word may be applied) of the mechanical motion as compared with the other. I thought at first that it might be attributed to the sameness of the motion—*i.e.*, to the circumstance that when the blows are inflicted mechanically, however gently, they are all in the same direction and each acts on a part which has been acted on before. As, however, I have not found that altering the direction of the motion makes any perceptible difference in the result, this explanation fails. What is to be learned from the observation is this: the very slightest touch is an "adequate" stimulus; any beyond this is more than sufficient and injurious. Its exhausting influence is, however, evidently perfectly local; for the loss of sensibility of one hair fails to determine any similar loss in others which have not been excited. If in any leaf a single hair, excited repeatedly by mechanical means, has ceased to respond, you can at once reproduce the result by acting on another hair, which will respond just as readily as if the leaf were intact.

Electrical excitation.

1. *Single induction shocks.*—If dry metallic electrodes are applied to the internal surface of the leaf, induction shocks conducted through them are without effect, however near the electrodes may be—the reason being, no doubt, that the surface is so perfectly dry that but little current passes between the platinum points. This difficulty may be overcome, either by using the pointed wires so that by piercing they can be brought into contact with the cells beneath, or by covering the surface with a layer of conducting material such as kaolin, made into paste with half per cent. solution of common salt. I used the former of these methods in my first experiments in 1873, as well as in those of which the results were published in 1876. For obvious reasons it is preferable to use non-polarizable electrodes, to which the form described in Part II. is the best that can be given. I have always used the small induction apparatus of DU BOIS-REYMOND without removing the core, and one DANIELL cell in the primary circuit. In all experiments opening induction shocks have been exclusively used. As a rule, the electrodes are applied to opposite points of the upper and under surface of one lobe. With reference to the action of such currents, so applied, it has been ascertained: (1) That when in a succession of excitations the secondary coil is

gradually brought nearer to the primary, it is observed that no effect whatever follows the passage of the induction current until the secondary coil is approximated to a relatively short distance, usually about 10 centims. from the primary. (2) That the excitatory effect produced at this distance is unmodified by further approximation. (3) That the distance of the secondary coil at which the first response takes place, when in a succession of excitations it is gradually approximated, is much greater when the induction current is directed from the upper to the under surface than in the contrary case; in other words, that the leaf responds to a weaker current when it is directed downwards than when it is directed upwards; and, finally (4) That if one current of an intensity a little less than sufficient to evoke a response be followed at a short interval of time by a second current of similar intensity, the latter may be effectual. This last fact will be reverted to further on, under the head of "Summation."

Excitatory influence of the voltaic current.

For the purpose of studying the influence of a weak voltaic current, a rheochord of platinum wire was used, of which the length was 14 feet and the resistance 20 ohms; this was interpolated in the circuit of a DANIELL cell. Currents could be derived from it and conducted through the leaf by connecting one electrode with the end of the rheochord, the other with any point of its length by a slider. In this case the derived current might, in consequence of the relatively high resistance of the plant, be regarded as proportional to the length of wire between the end block and the slider. By comparing this rheochord with the compensator it was found that each foot of wire corresponded to a tension difference of about 0·06 D. and hence the whole wire to about 0·84 D. With this contrivance the following observations were made. In the first series the leaf was led off to the galvanometer as in the fundamental experiment, but a switch (a POHL's Wippe, without cross wires) was introduced into the circuit, of such construction that for one or more seconds a derived current from the rheochord could be sent through the electrodes, the galvanometer circuit being for the same period broken. As care was taken to compensate immediately before the switch was thrown over, this produced no movement of the needle, but served to show in what way the passage of the current modified the electromotive properties of the leaf. The effects observed were as follows:—(1) When a very weak current was used (1 foot of wire, corresponding to a difference of potential of 0·06 D.) a slight effect, resembling polarization, was observed—*i.e.*, a transitory deflection indicating the existence in the leaf of a current opposed to the current which had been led through it, its direction being upwards when the anode was on the upper surface, and *vice versâ*. It was ascertained that this was not due to polarization of the surfaces of contact of zinc with zinc sulphate solution, for when the experiment was repeated with the clay points in contact with each other, *i.e.*, without the interposition of the leaf, no deflection was observed. (2) When the derived current was doubled (2 feet of wire) the after effect was always positive, whatever was the direction of the current. In other words, the effect showed itself in either case as an augmentation of the previous difference. Consequently the downwards current left behind it as its after effect, a current in the same direction as itself. By increasing the current, this effect, whether its direction is upwards or downwards, could be increased in about the same proportion. When the experiment was made in a leaf of which the opposite lobe was led off to the electrometer, no effect was observed during or after the passage of the current, showing that the effect was not propagated, that is, was not an excitatory effect and did not cause one. When similar currents were led in a similar way through the leaf stalk by electrodes on opposite surfaces, a polarization took place, but no effect resembling the one described was observable. (3) When (1) and (2) were alternated, that is when the current was passed through the leaf a number of times in succession in the same direction, but alternately with different strengths of current (1 foot and 2 feet of wire), the effect was, of course, alternately positive (with the stronger current) and negative (with the weaker). In both cases the effect was evanescent, and the previous difference was unaffected. (4) When the length of wire was increased to about 5 feet (a current five times as great as in (1)), different lengths, however, being required in different leaves, the effect was that of an excitation, that is, there was a negative deflection which was cut short by a larger one in the opposite direction. When this was compared with that of a mechanical excitation of one of the hairs of the opposite lobe the two variations, although resembling each other, were not identical. For if in a succession of observations a momentary (tenth of a second) closure of a downward current of sufficient strength to excite the leaf was alternated with mechanical excitation of a hair of the opposite lobe so as to compare the effects, it was found that in the variation which followed the electrical excitation, the first phase was smaller, the second phase larger than in those produced mechanically.

	Mechanical.		**Electrical.**
	Previous difference.	**Variation.**	**Variation.**
1st series	+0·0003	− 36 +192	− 3 + off scale (*i.e.*, over 600)
2nd series	+0·0028	− 67 +132	−10 + ditto
3rd series	+0·0035	−102 +110	−11 +500

Each series consisted of six mechanical and as many electrical excitations. The mean results of each series are given in the columns headed "Variation."

(5) The excitatory effect produced in (4) was followed by an "after effect" which subsided very gradually in the ordinary way, leaving behind it a lasting increase of the previous cross difference, and when a leaf was subjected to successive excitations, the

variation gradually changed its character just as it does when mechanical excitations follow each other at short intervals. In a leaf of which the cross difference was, at the beginning of the experiment, abnormal, and which was excited 58 times in succession in half an hour, the variation being observed alternately with the electrometer and galvanometer, the cross difference changed sign from −0·009 D. to +0·005 D. The mean difference and variation were in three successive periods as follows:—

	Cross difference.	Galvanometer.		Electrometer.	
1st period	−0·0023 D.	−40·8	+137	−29·8	+1·8
2nd period . . .	0	−51·0	0	−23·2	0
3rd period	+0·0036 D.	−61·4	0	−21·0	0

In another similar experiment the cross difference changed, in a series of 32 excitations, from −0·011 D. to +0·01 D., the second phase (positive deflection) diminished from +210 scale to +90 scale, while the first phase (negative) increased from −15 to −220. (6) When (3) was repeated with the difference that the lengths of wire were respectively 3 feet and 5 feet, instead of 1 foot and 2 feet in two series *a* and *b*, so that the currents conducted through the leaf were, in series *a*, only sufficiently strong to produce the effect (2), viz.: a single positive deflection, whereas in series *b* they were sufficient to give rise to an excitation, it was observed that in series *a* the electrical relation between the upper and under surface remained unaltered, but that in series *b* the diphasic variation was followed by an after effect which subsided with the usual increase of the cross difference already described and exemplified.

The preceding facts may be summed up as follows:—The closing of a voltaic current directed from the upper to the under surface of the leaf of Dionæa produces different effects according to its strength. Very weak currents give rise to feeble polarization. Currents of moderate strength produce a transitory increase of the cross difference, that is, make the under surface more positive to the upper surface than it was before. Strong currents produce this transitory increase of the cross difference, but, in addition, produce an excitation which is followed by a lasting increase of the cross difference. In the successive deflections observed, these two effects are summed; so that the negative phase is diminished and the positive phase increased.

The explanation of these phenomena depends on the meaning to be assigned to the remarkable modification of the electrical state of the leaf produced by moderate currents. On this point we can only express ourselves negatively. It is not analogous to an excitatory effect, for it is not capable of propagation: it is not identical with the after effect for it is transitory: it is not a polarization effect, for its direction is always the same as that of the cross difference whatever the direction of the current which produces it. This fact also makes it impossible to identify it with KUNKEL's currents. The utmost that can be stated with reference to it is, that it indicates the occurrence of a local change in the path of the current, which is probably of the same nature as that which, when stronger currents are used, excites a propagable explosion.

By the experiments already related it has been shown that when a leaf is traversed by a voltaic current in a direction vertical to its surface for a short period (one-tenth of a second to a second) from the upper to the under surface, excitation follows as soon as the difference between the two electrodes applied to the two surfaces of a leaf at opposite points, amounts to something like half the electromotive force of a DANIELL cell. But they afford no information either as to the time or place at which the excitation occurs. It remains to be determined whether it occurs at the closure or opening of the circuit, and whether at the anode or cathode. The first of these questions was easily decided in experiments in which the leaf was led off as in the fundamental experiment and excited by currents for periods of sufficient length to render it possible to distinguish easily between the effects of opening and closing. The method consisted in making a series of observations in which the current was alternately directed upwards and downwards, and lasted from five to seven seconds, the opposite lobe being led off to the electrometer. A succession of experiments were made on different leaves in which various strengths were used, from one DANIELL, without rheochord, to two GROVES, always with the same result, namely, that the leaf responded at make, and at make only, whatever was the strength of the current or its direction. When strong currents were used, as will be noticed further on, the excitation at the closure of the circuit was succeeded by another from three to six seconds later, and might be easily mistaken for a break effect, if the moment at which it occurred happened to follow that at which the current was opened.

To the question whether excitation proceeds from the anode or cathode, we have failed entirely in our efforts to obtain an answer. The modes of experiment used were as follows:—The leaf having been led off to the electrometer as in the fundamental experiment, non-polarizable electrodes for excitation were applied on the surface of the opposite lobe and on the petiole. In some cases the lobe electrode was on the under surface, in others on the upper. A reverser was introduced into the exciting circuit, so that the current of two GROVES could be directed either towards the leaf or towards the leaf stalk. In either case the excitatory effect showed itself at make, and make only. But when the electrode was on the upper surface it was anodic, and when it was on the lower surface, cathodic. At first sight it seemed as if this indicated a difference of action between the poles, but on consideration it appeared to be another form of the result already recorded, viz.: that in the lamina the downwards current is more effectual than the upwards. A current *from* the leaf stalk which passes out at the under surface acts in the same way as a current *to* the leaf stalk which enters at the upper surface, because in the excitable part the direction of both is descending. The next attempt to settle the question was by a rheochord experiment, in which the leaf was excited by electrodes applied symmetrically to corresponding surfaces on the right and left lobes respectively, the one lobe (right or left as the case might be) being led off to the galvanometer as in the fundamental experiment. The time of closure of the exciting circuit (two LECLANCHÉ'S) was one-twentieth of a second. The galvanometer circuit was also closed for the same period, an interval of 0·03" intervening between the opening of the one circuit and the closure of the other. In the first leaf experimented on, the results were remarkable. In a series of twelve observations in which the current was directed across the leaf, alternately from and to the led off lobe, the needle remained motionless when, as in the first case, the anode was next the leading off surface, but swung to the negative side from 12 to 14 scale when the cathode was in the same position. As repetition confirmed the result it was clear that, as regards the particular leaf under observation, the proximity of the cathode was favourable to the early occurrence of the excitatory effect. Doubt was, however, suggested by the consideration that here, when the cathode was on the left lobe, the direction of the current near the leading off electrodes was downwards, that is, favourable to the excitatory effect, whereas in the opposite lobe it was unfavourable, and this surmise we confirmed by the observation that in another leaf, in which an exactly similar experiment was made, with the exciting electrodes applied to the internal surface, the response was strongest when the anode was on the led off lobe. All that was learned from these attempts was, that by the method employed, which seemed to be the only one likely to yield an answer to the question, no answer could be obtained. The want of material made it impossible to proceed further, but even had it been forthcoming I should have been at a loss how to use it.

Influence of the Duration of the Current on the Effect.

1. *Short periods.*—In order to determine the shortest period of closure of a voltaic current directed transversely from the upper to the under surface of a leaf, experiments were made in which the time of closure was gradually shortened by means of the pendulum rheotome. It was found that there was no diminution of effect until the duration of the closure was less than one-hundredth of a second. Beyond this, the response became uncertain when two DANIELL'S with a rheochord of 20 ohms were used, and disappeared when the closing time was reduced to 0·007. Want of material made it impossible to investigate the important question of the influence of strength of current on the minimum. It was, however, ascertained that it was necessary to lengthen it when weaker currents were used.

2. *Currents of long duration.*—When a voltaic current of five seconds' duration and of moderate strength is led through the leaf from the upper surface between the sensitive hairs to the under surface, there is a response at make, and no other. When stronger currents are used (from one DANIELL to two GROVES) the first variation is followed by a second. If the current is continued for a long period (30 seconds) a succession of excitatory variations takes place at irregular intervals, as shown in the following

examples. If the leaf is expanded and not restrained, it may close at the first excitatory variation, *i.e.*, one to one-and-a-half second after the closure of the current. But if the experiment is made at a low temperature (18° to 20° C.) this event does not happen until after the third, fourth, or fifth variation. To observe these facts the current of a GROVE cell is led through one lobe, while the other is led off (in the usual way, to the electrometer, and the time after closure of the circuit noted by the watch, as in the following experiments :—

I. Current of one GROVE from above downwards for half a minute; excitatory variation at 3″, 9″, 13″, 19″, 23″, 30″.

Same current directed upwards—variation at closure only.

II. Current of one GROVE from above downwards for one minute; variation at 1″, 15″, 25″, 34″, 38″, 45″.

Same current directed upwards—variation at closure only.

III. Same leaf. Current directed through contacts previously used for leading off; leaf led off by lobe previously excited. Variation at 2″, 8″, 22″, 32″.

Same current in opposite direction—variation at closure only.

Summation of stimuli.—To prove summation it must be shown that two equal excitations, each of which alone is inadequate to evoke a response, do so by summation, that is, when they follow each other at a short interval. If this is so, there is evidence that the first, although it is apparently without effect, yet produces a change in the excitable structure which renders it more excitable than it was before. The best method consists in making two alternating series of excitations, in one of which single opening induction shocks, just inadequate to produce excitatory effect, are used, while in the other, two induction currents of equal strength follow each other at a variable interval.

1st Series. Distance of coil 7·6 centims. Temperature of chamber 32° C. Interval 0·02.″ Ten excitations, of which Nos. 2, 4, 6, 8 were single, 1, 3, 5, 7, 9 were repeated. All of the latter were effectual; all of the former ineffectual.

2nd Series. Distance of coil 7·4 centims. Temperature 32° C. Interval 0·1.″ Ten excitations; six single, five repeated ($\frac{1}{10}$″). All of the latter were effectual; all but one of the former ineffectual.

3rd Series. Distance of coil 7·3 centims. Temperature 32° C. Interval 0·2.″ Sixteen excitations, of which half single, the rest repeated ($\frac{1}{5}$″). All of the latter were effectual; all except two of the former ineffectual.

4th Series. Another leaf. Distance of coil 8 centims. Temperature 32° C. Four excitations; two single, two repeated at intervals of $\frac{1}{8}$″. Both of the latter effectual; the other ineffectual.

Four excitations; two single, two repeated at intervals of $\frac{2}{8}$″. The latter effectual; the former ineffectual. In this series it could easily be seen that the excitatory effect followed the second excitation.

5th Series. Eighteen double excitations at intervals of half a second; of these eight were fruitless. Of the ten responses, half occurred at the first excitation, the remainder at the second.

These experiments show that at all intervals between 0·02″ and 0·4″ summation occurs, and that at 0·5″ the result becomes uncertain.

Another method may be used which consists in subjecting a leaf to repeated excitations by successive series of inadequate induction shocks at a fixed interval—*e.g.*, at a twentieth of a second, gradually increasing the number in each series until a response is obtained. In an experiment of this kind a leaf responded in a room at 20° C. when the number of opening shocks amounted to 23. In the chamber at 32° C. it responded after 7. It is to be noted that, in an experiment of this kind, the distance of the secondary coil from the primary must very slightly exceed that at which single shocks are responded to without fail. This method is useful as a means of testing excitability under different conditions.

PART VI.

RELATION BETWEEN THE EXCITATORY PROCESS AND THE MECHANICAL EFFECT.

The moment at which the mechanical effect begins can be easily measured graphically by the following method. A cork is prepared, with a hole across its axis for the reception of the round horizontal arm of a support resembling that shown in fig. 4 (electrodes). On this the cork can be rotated. The cork is cut so as to present a surface somewhat smaller than that of a lobe of the leaf. To this a leaf is cemented with plaster of Paris by the external surface of one lobe, leaving the midrib free. To the border of the opposite lobe a very light straw lever is fixed by the same means, in the position it would assume if it were a prolongation of the middle marginal hair. This having been done, the lever is made horizontal by rotating the cork which, with the leaf, is brought into such a position that its motions are inscribed on a blackened cylinder, of which the horizontal movement should be 1 centim. per second. The leaf is excited mechanically by a camel-hair pencil of which the motion breaks a circuit. The moment at which this happens is recorded on the same cylinder by a DEPREZ' time marker. It is thus learnt that the mechanical response, at temperatures from 15° to 20° C., begins at an interval of from a minute-and-a-half to two minutes after excitation, so that in the fundamental experiment it coincides with the close of the first phase of the variation. If this observation is made at a low temperature, as *e.g.*, 15° C., and the hairs are touched with great care, the first excitation produces no perceptible motion. At the fifth or sixth the lever slowly rises and continues to do so for several seconds (see Proceedings 1876, p. 415). To record this effect, the best plan is to leave the lever free and record its position at the end of every half second on a smoked glass plate. The result is shown in the figure. It is seen that in each case the rate of ascent of the lever is greatest during the first half second, after which it gradually declines, and that the total effect consequent on each excitation is very variable, being dependent on accidental differences in the way in which the sensitive hair is touched.

The mirror experiment.—The effect of series of frequently repeated mechanical excitations on leaves at relatively low temperatures may be observed with greater accuracy by another method. The leaf is attached to the rotating cork in exactly the same way as in the experiment last described, but instead of a lever a very light glass mirror, such as is used for reflecting galvanometers, is cemented to the external surface of the free lobe, near the margin. By means of this mirror the image of a horizontal slit is thrown on a vertical scale which is so graduated that the angular movement of the lobe can be measured with great delicacy. An experiment of this kind gives the following results. The leaf having been led off to the electrometer by electrodes applied to the upper surface of the attached lobe and to the midrib, was subjected to 22 excitations, each of which consisted in touching very gently one of the hairs of the attached lobe. The total angular movement (rotation) of the mirror was 167°. This was accomplished in 20 excitations, all of which were effectual in the sense that each produced a normal variation.

Number of excitation.	Effect.	Sum of effects.
1	°·0	°·0
2	·0	·0
3	0·5	0·5
4	0·4	0·9
5	0·4	1·3
6	0·8	2·1
7	1·0	3·1
8	4·1	4·5
9	3·5	8·0
10	4·0	12·0
11	5·5	17·5
12	7·5	25·0
13	13·0	38·0
14	15·0	53·0
15	42·0	95·0
16	34·0	129·0
17	10·0	139·0
18	11·0	150·0
19	13·0	163·0
20	4·0	167·0

The fact that in this and in other similar experiments it is possible to excite the leaf once, twice, or a greater number of times without any appreciable movement of the image, at first sight seems to indicate that the electrical effect is independent of the mechanical. But before accepting such an inference it must be remembered that we cannot be at all sure that the interstitial movement of liquid, which in the leaf as in all the moving organs of plants is the efficient cause of change of form, may not begin without making itself visible by any change in the curvature of the lobe, however delicate our means of observation may be. It appears to me probable that every effectual excitation—every excitation which is marked by electrical disturbance—is also followed by diminution of the water-charge of the excited protoplasm. At first, this may either expend itself entirely or produce only such small effects as, in the experiment last related, presented themselves in excitations 3 to 7.

In our previous paper the phenomena described above were referred to as evidences of summation. It will be better in future to limit the application of this word to the still more remarkable phenomenon which has been described under the heading "Summation" in Part V., p. 47; for those phenomena are strictly analogous to the effects to which the term "Summation of stimuli" has been applied by STIRLING.* The term "Summation" is in itself as applicable to the process now under consideration as to the other, but it differs from it in this respect—that it is rather a summation of effects than a summation of excitations. The process with which it is most comparable in the physiology of animal excitable tissues is that which is known as the *Treppe* or staircase.† By this term is designated the fact that when the ventricle of the heart of the frog is subjected, after a period of rest, to a series of effectual excitations, the contractions become stronger and stronger, although the excitations are all of equal intensity. In both processes the mechanical effect of the first excitation is much smaller than that of the second, the second smaller than that of the third, and so on, but in the *Treppe* the difference which is large at first rapidly diminishes, whereas in the leaf it as rapidly increases. Further, the total mechanical effect of the first excitation is so small as to be barely perceptible, whereas in the heart the first contraction (*i.e.*, the weakest) is at least half as vigorous as the strongest. But in proceeding with our comparison we find that in both, the change of form is opposed by a resistance which has its seat in the structure of the organ, and is removed step by step when it is subjected to repeated excitations. In the heart this resistance diminishes rapidly at first, more slowly afterwards: in the lamina of the leaf it does not begin to yield until after several excitations, provided that they are only just strong enough to provoke an electrical response. This difference is unimportant, and becomes almost imperceptible if somewhat stronger excitations are substituted for the minimal ones. In this case the effect of the very first excitation may be large, that of the second larger, the third still larger, and so on, but with decreasing, not increasing increments, just as in the *Treppe*. The most fundamental difference is that which concerns the relation between the resistance and the mechanism of contraction in the two cases. In the ventricle only a fraction of the mechanical effect, even of the first contraction, is due to abolition of resistance. At each repetition of the excitation this fraction diminishes, until after a dozen or so of excitations the successive mechanical effects become sensibly equal. From the first, therefore, the change of form is due to something more than mere diminution of resistance. In the leaf the removal of resistance is everything. Consequently, so soon as a sufficient number of excitations have occurred to accomplish this, it is not only incapable of resuming its original form, but of any further response to excitation until, after many hours, the resistance is restored.

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* STIRLING, "Ueber die Summation electrischer Hautreize." LUDWIG'S Arbeiten, 1874, p. 223.

† See BOWDITCH, "Ueber die Reizbarkeit der Muskelfasern des Herzens." LUDWIG'S Arbeiten, 1871, p. 156.

If we go a step further and enquire what this resistance consists of, we are led, in accordance with the conclusion that has been arrived at by every physiologist who has investigated the mechanism of the changes of form of the excitable organs of plants, to identify it with turgescence. By turgescence we understand the power which living protoplasm possesses of retaining water. In the case of cells which are excitable, the immediate effect of excitation is suddenly to diminish this power, and thereby to produce a diminution of volume of the cells which is equal to that of the water (probably holding diffusible bodies in solution) which is discharged into the intercellular spaces.

ADDENDUM.

Received November 3, 1881.

[*The following is a Summary of the most important Experimental Results embodied in Parts III., IV., and V.*]

1. When different parts of the surface of the uninjured and unexcited leaf of Dionæa are compared by the method of compensation described in Part II., electrical differences present themselves even when the points selected are symmetrical. In this case the differences observed are accidental. They are due either to surface conditions which disappear when the leaf, with its leading off electrodes in position, is allowed to remain in saturated air (differences of turgidity of the surface layers of cells) or to accidental physiological differences between the two lobes, which do not disappear.

2. Similarly, when unsymmetrical points of either surface of the leaf are led off, the differences observed are for the most part accidental. In one case, however—that of the comparison of the middle of the under surface of either lobe with the under surface of the petiole—the negativity of the former to the latter is a result which occurs so constantly that it must be assumed to have a physiological meaning.

3. If any two points opposite to each other on the upper and under surface of either lobe are compared, it is usually found that the upper surface is negative to the lower. Whether this is so or not in the unexcited leaf, it becomes so after one or more excitations; in other words, the effect of mechanical or electrical excitation, whether applied in the neighbourhood of the surfaces compared or at a distance from them, is to produce a lasting change in their electrical relation of such a nature that on repeating the comparison of the two surfaces the *under surface* is always *found to have become more positive* (*less negative*) *than it was before excitation.*

4. If the same observation is made while the leaf is subjected to mechanical or electrical excitation, it is seen that at the moment that excitation takes place the under surface becomes suddenly negative to the upper. By the rheotome we learn that the change last mentioned does not begin until $\frac{1}{20}$th of a second after its cause, that it culminates about the middle of the first second, the maximum difference amounting to as much as 0·08 Daniell, and then gradually subsides.

5. It is further learnt by the same mode of observation that in the course of the second second or towards its close, the electrical relation of the surfaces to each other is reversed, the under surface becoming positive to the upper. The difference of potential in this direction having rapidly augmented until it amounts to about 0·02 Daniell, very slowly subsides into the lasting effect of excitation described in 1.

6. When two symmetrical points on the under surfaces of opposite lobes are similarly compared during excitation, the excitatory effect differs in character according to the relation between the seat of excitation and the leading off contacts, the surface of the lobe which is directly excited becoming negative to the other, consequently by changing the positive seat of excitation the direction of the first effect may be reversed.*

7. When a voltaic current is led across the lobe from the upper to the under surface by electrodes applied opposite to each other near the sensitive hairs, but not touching them, at the same time that the electrical state of the opposite lobe is observed as in 4, a response occurs at the moment that the current is closed, provided that its strength is such that the available electromotive force amounts to about half that of a Daniell cell and that the temperature is not below 30° C. No response occurs at opening the current. If a stronger current (two Daniells) is used and the direction is downwards, the response at closing the current is followed by several others. This effect does not happen when the current is directed upwards.

8. A voltaic current directed from the upper to the under surface, which is too weak to evoke an excitatory response, produces an increase of the positivity of the under surface, limited to the part of the lobe through which the current passes, which lasts several seconds after the current is broken. Its direction shows that it is not due to "polarization."

9. Voltaic currents of less than $\frac{1}{200}$th second duration, though of moderate strength, do not excite Dionæa. Weaker currents cease to act when their duration is less than $\frac{1}{100}$th second. But the relation between strength and duration has not been ascertained.

10. Opening induction currents such as are yielded by du Bois-Reymond's smaller inductorium when the secondary coil is at a distance of 8 to 10 centims. from the

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* It cannot be concluded either from the result stated above or from the preceding ones that in Dionæa, as in muscle and nerve, the excited part becomes *negative* to other parts. For it is as consistent with the facts observed to say that the upper surface becomes positive as the immediate result of excitation, as to say that the under surface becomes negative.

primary, may for many purposes be used more advantageously than any other mode of excitation, for by means of them a leaf may be subjected to repeated excitations for several hours without failure or appreciable exhaustion.

The minimum effectual strength of an opening induction current depends on its direction, weaker currents being responded to when they are directed downwards than in the contrary case. Induction currents which are inadequate to evoke a response are yet followed by a local change in the electrical state of the surfaces through which they are led which resembles that produced by weak voltaic currents.

11. *Summation of stimuli.*—Two inadequate opening induction currents which follow one another at any interval greater than $\frac{2}{100}''$, or less than $\frac{4}{10}$ths of a second, may evoke a response. In this case the response follows the second excitation. When a leaf is subjected to series of inadequate induction currents at short intervals (*e.g.*, $\frac{1}{20}$th of a second) the response may occur after a greater or less number of excitations, according to the temperature at which the experiment is made and the strength of the current.

12. *Summation of effects.*—In a series of mechanical excitations each of which is just adequate to produce an electrical response, those which occur earliest are followed by no visible change of form. Of the later members of the series each produces a measurable movement, the extent of which becomes greater each time that the excitation is repeated until eventually the leaf closes.]

CONCLUSION.

In the preceding paragraphs all reference has been omitted to the physiological meaning of the electromotive phenomena which have been described. The moment has now arrived at which it appears necessary to offer such an explanation as the knowledge acquired justifies, as to their relation to the vital processes with which they are associated.

According to Professor MUNK the electromotive properties of the leaf of Dionæa may be accounted for on the supposition that in each cell the ends of the cell are, in the resting state, positive to the middle, and that in excitation the difference of potential between the ends (poles) of the cell and its middle (equator) undergoes a sudden diminution. This theory must be rejected at once, on the ground that it fails to explain the fundamental experiment. The electromotive forces it supposes to be in operation act in directions parallel to the surface of the leaf—at right angles, therefore, to the path of the currents which show themselves when the two opposite surfaces of the leaf are led off to the galvanometer. Under these conditions it is impossible that the latter can be the expression of the former.

On the other hand, I accept as fundamental the doctrine that whatever physiological properties the leaf possesses, it possesses by virtue of its being a system of living cells. The first question, therefore, to be determined is that of the electromotive endowments of the individual cell.

Each of the two lobes of the leaf of Dionæa is made up, for the most part, of cylindrical cells. The long axes of these cells (parenchyma cells) are all parallel to each other, and to the vascular bundles which run out from the midrib towards the margin. A transverse section of the leaf, made in such a way as to include a vascular bundle, shows that there are two or three layers of them, the most superficial of which are covered by the epidermis of the under and upper surfaces of the leaf. As they are alike in structure, it may be assumed that they have the same or similar functions.

If it were possible, without injury, to investigate the electromotive properties of one of these cells individually, by leading off its opposite surfaces, we should probably find them to be isoelectrical both at rest and when excited; for the electrical differences which exist in the cell are probably between the living inside and the non-living outside, and not between different parts of the surface. Consequently the electrical differences which show themselves between the opposite sides of the leaf blade which is made up of layers of such cells cannot be due to the summation of smaller differences between the two sides of the individual layer, for these are isoelectrical. They must, therefore, be referred to the contact of different layers of cells with each other. But if electrical differences exist at the surfaces of contact of the cells, they must be associated with physical differences of other kinds. With respect to the nature of this difference it appears to be very probable that the most important element is migration of water. For on the one hand we know that in consequence of the surface evaporation, migration of water certainly exists, while on the other we have proof in the experiments of Dr. KUNKEL, that such migration cannot occur without producing electrical differences.

As has been so conclusively proved by the experiments of PFEFFER, those motions of the organs of plants which follow excitation are attended by diminution of the turgor or water-charge of the protoplasm of the excitable cells. The displacement of water occasioned by this discharge must unquestionably be attended by electrical change. In Dionæa the mechanical effect of an excitation can be seen from one to two minutes after the moment at which the excitation takes place. It is at the same moment that the second phase, or after effect, begins. In their origin, culmination, progress, and duration the two changes are closely associated. It therefore appears extremely probable that the two are causally related—that the positivity of the external surface is an expression of the electrical differences which exist between contiguous strata in different degrees of turgor—the less charged being negative to the more.

The same theory affords us a complete and satisfactory explanation of the gradual change of potential which always results from repeated excitation, as well as for the fact so constantly observed, that the "after effect" which follows each of the first few excitations to which a leaf is subjected, is much greater in extent than those which occur after later ones, and that eventually the after effect fails entirely, reappearing after a period of repose.

I need scarcely say that the first phase of the variation—the effect which immediately follows excitation and has an opposite sign to the after effect and a much higher electromotive force—does not admit of a similar explanation; for it cannot be imagined that a change which spreads over the whole lamina in less than one-twentieth of a second can be dependent on migration of water. The excitatory disturbance which immediately follows excitation is an explosive molecular change, which by the mode of its origin, the suddenness of its incidence, and the rapidity of its propagation is distinguished from every other phenomenon except the one with which I have identified it, namely, the corresponding process in the excitable tissues of animals.

Of the nature of this preliminary disturbance (to which alone the term excitatory variation ought to be applied, it alone being the analogue of the "action current" of animal physiology) we know nothing. Just as in the case of muscle and nerve, the proof that it is attended by any chemical alteration or by the separation of any product of disintegration is wanting, so it is here. In the one case as in the other, we must regard the electrical change as a visible sign of an unknown molecular process. That it is not the primary change which occurs in protoplasm when it enters into the state of excitation we have evidence in two facts, first, that even when the exciting agent is an induction current which passes through one of the opposite surfaces by which the leaf is led off to the galvanometer, so that no time whatever is lost in transmission, there is still an interval between excitation and response of about 0·03″, during which interval molecular changes are obviously in progress; and, secondly, that excitation just insufficient to evoke a response gives effect to a second which follows it, provided that the interval between the first and the second does not exceed one-third of a second—a result which can only be explained on the supposition that the first excitation actually effectuates a molecular change, notwithstanding that the visible sign is wanting.

The direction of the excitatory effect in the fundamental experiment is such as to indicate that in excitation, excited cells become positive to unexcited, whereas in animal tissues excited parts always become negative to unexcited. The apparent discrepancy will probably find its explanation in the difference of the structural relations of the electromotive surfaces.

II. Lucifer: *a Study in Morphology.*

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[Plates 1–11.]

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Section I.—Introductory.

The general anatomy of the adult *Lucifer* has been satisfactorily made known by the observations of Souleyet, Huxley, Hensen, Dana, Semper, Claus, Dohrn, and Faxon; and the only facts which I have to add relate to the structure of the reproductive organs.

The earliest recorded observations upon this subject are by Dana ('United States Exploring Expedition during the Years 1838, 1839, 1840, 1841, and 1842,' under the Command of Charles Wilkes, U.S.N., vol. xiii., part 1). In plate 44, fig. 9, *b*, *h*, and *m″*, he gives a very correct representation of the male reproductive organs and spermatophore of an adult male specimen of *Lucifer* (*acestra*); but his description of these figures (p. 670) shows that he was completely at a loss for an interpretation of the parts which he has represented, and had no idea of their true function.

Later students have entirely overlooked these figures by Dana, and there has been some doubt whether *Lucifer* is an adult animal at all, rather than the young form of some other Decapod.

In 1861, SEMPER (Reisebericht des Herrn Dr. SEMPERS. Ein Schreiben an A. KÖLLIKER; Zeit. f. Wiss. Zool., xi., 1861, pp. 100–108) re-discovered and described the male organs, and also the female organs, of a large, transparent, and probably new species which he found at Zamboanga.

He gives no figures, and his short account, which is in the main correct, is as follows:—

"Die Geschlechtsöffnung ist einfach, liegt bei beiden Geschlechtern in der Mittellinie des Bauches, dicht hinter dem letzten Brustfusse. Der Hode besteht aus einer, in der Mittellinie des Thorax, dicht unter dem Magen liegenden Samendrüse, an deren hinteres Ende, dort wo der kurze Samenleiter entspringt, sich mehrere Nebendrüsen ansetzen. Der Same wird, noch unentwickelt, in einen birnförmigen grossen Spermatophor eingeschlossen. Das hinterste Ende dieser männlichen Drüse reicht bis in die Mitte des ersten Hinterleibgliedes, das vorderste bis ziemlich dicht an den Schlund.

"Das Weibchen hat zwei Eierstöcke, die vom Ende des sechsten Hinterleibgliedes an dicht unter dem Darm, sich bis in die Mitte des Thorax erstrecken, hier biegen sich die beiden Samenleiter nach unten, und schwellen dann zu zwei grossen Taschen an, die eine kleine Tasche umfassen; die Geschlechtsöffnung ist einfach; ein einziger Spermatophor steckt mit seinem spitzen Ende darin. Entwickelte Zoospermien habe ich nicht beobachtet. Weibliche Begattungsorgane fehlen. Die Entwickelungsgeschichte ist mir unbekannt geblieben."

The male organ has two external openings; they are not on the median line, and their position in the body does not correspond to that of the female orifice; but in other respects my own observations show the correctness of this description.

As SEMPER does not give any account of the general structure of these sexual individuals, CLAUS ("Ueber einige Schizopoden und niedere Malacostraken Messinas," Zeit. f. Wiss. Zool., xiii., 1863, pp. 433–437) held that the adult nature of *Lucifer* must still be a matter of uncertainty; but in 1871 DOHRN verified SEMPER's account from alcoholic specimens ("Untersuchungen über Bau und Entwickelung der Arthropoden," von Dr. ANT. DOHRN, Zeit. f. Wiss. Zool., xxi., 1871, p. 357), and showed that the mature animals have the form which had been described by THOMPSON ('Zoological Researches and Illustrations,' 1829, p. 58) as characteristic of the genus.

In the following year SEMPER ("Zoologische Aphorismen, von C. SEMPER: I. Einige Bemerkungen über die Gattung *Lucifer*," Zeit. f. Wiss. Zool., xxii., 1872, p. 305) published a second paper, in which he gave two good figures of the male and female reproductive organs (taf. xxii., figs. 3 and 4), but added nothing to his earlier description.

During my own studies upon the development of the larva I found an abundant supply of adult specimens of both sexes, and am thus enabled to give a more complete account of the structure and relations of the reproductive organs.

Plate 9, fig. 75, is a side view of the carapace (*c*) and the first abdominal somite (*a*) of an adult male, showing the first abdominal foot (*Pl.* 1) and the basal joints of the third maxilliped (*Mp.* 3), and the first, second, and third thoracic limbs (*Pr.* 1, *Pr.* 2, *Pr.* 3). The testis (*t*) consists of a series of about eight pouches or follicles, which hang down into the body cavity under the anterior end of the intestine (*i*). The body of the animal is so thin that it is almost impossible to get a good dorsal view without crushing the specimen; but a very careful examination of the side view seems to show that there is only a single organ on the median line of the body, as SEMPER states. On each side of the intestine, along the line where the testis joins its wall, a small tubular vas deferens (*vd*) arises, and runs backwards along the side of the intestine nearly to the end of the first abdominal somite, to which it seems to be attached (at *l*) by a ligament. It then bends outwards and forwards upon itself to form a second much larger portion (*sp*), which is parallel to and outside of the first portion, and reaches nearly to the anterior edge of the first abdominal somite. The third or terminal portion (*sv*) has a large cavity, thick walls, and it runs down to an external opening which is situated on the outer edge of the sternal surface of the thoracic region, behind the basal joint of the third pereiopod, and therefore in the position which would be occupied by the basal joints of the fourth or fifth pereiopods if they were present.

There is a vas deferens, made up of these three portions, on each side of the body, and the ventral nerve chain (*tg*) passes between their external openings.

The more anterior follicles of the testis are almost perfectly transparent, but the development of the male cells in the posterior ones gives to them a faint granulation. The first division of the vas deferens (*vd*) has a small cavity, thin walls, and as it usually seems to be entirely empty it is probable that the passage of the male cells from the testis through it to the second division (*sp*) takes place quickly. The second division (*sp*) has a very large cavity, and in it the male cells become arranged in a single layer around the surface of a central core, which is formed of some dense transparent adhesive substance.

The spermatophore appears to pass into the third chamber (*sv*) before it is completely formed, as all those which were seen in the second chamber consisted only of a central core and a layer of male cells, while those which were contained in the thick-walled third chamber had an outer enveloping capsule.

I found several specimens with a fully-developed spermatophore on one side of the body and none on the other side, and was thus enabled to thoroughly satisfy myself of the presence of two vasa deferentia, and two external openings.

I was unable to discover how the spermatophore is transported to the body of the female, or what part the clasping organ (*c*) upon the first pleopod of the male performs during the act of copulation.

Upon several occasions I observed a male clinging to the basal joints of the first antennæ of a female, but as I never succeeded in getting the pair under a lens without separating them, I made no careful examination. Copulation usually takes place during the daytime, or at least this was the case in every instance which I observed. In several cases I found female specimens with a single fresh spermatophore attached to the opening (Plate 9, fig. 74, *o*) of the seminal receptacle (*sr*). This opening is situated between and a little anterior to the basal joints of the third pair of thoracic limbs (*Pr.* 3 of fig. 74). As the spermatophore gradually discharged its contents, it was easy to see that both the central core and the investing layer of spermatozoa escaped from the outer sheath and passed into the seminal receptacle. In all the breeding females which I observed the spermatozoa filled the posterior, and the transparent core of the spermatophore the anterior half of the spermatic receptacle, as shown in fig. 74. The ovary is very long (fig. 74, *ov*), and it lies under the intestine, reaching from the fifth abdominal somite to the posterior edge of the carapace, where it bends upon itself at right angles and runs down to its external opening, which is upon or close to the median line of the ventral surface, a little in front of the third pair of pereiopods. The wall of the ovary is so very thin and delicate that I was not able to detect it at all except when it was filled with ripe ova. These are very much elongated, granular, and slightly opaque; and there does not seem to be any shell around them. They are very elastic, and undergo great changes of shape as they pass through the small oviduct.

Oviposition occurs between 9 and 10 o'clock in the evening, and occupies only a few minutes.

After the eggs are laid they are spherical, transparent, and each one has a rather thick shell. They are attached, in a loose bunch of twenty or more, to the last pair of thoracic limbs, and in order to save space I have shown them in fig. 74, although the specimen from which the figure was drawn had not laid any of its eggs.

As I obtained very few ripe females, I was not able to sacrifice one of them to study the reproductive organs under pressure, and I am therefore unable to decide whether any parts of this system are double; but I feel confident that there is only one spermatic receptacle, and the opening of the oviduct seems to be upon the median line.

We found a few adult specimens out at sea, but, while I was able to learn little about their habits, I think that they are not strictly pelagic, but that their proper home is the salt marshes close to the ocean.

They were met with in the greatest abundance about half-a-mile inside Old Topsail Inlet, near a large marsh, during the first hour of the ebb tide, on calm evenings when the tide turned between 7 and 8 P.M.; and I infer that they leave the marshes at this time to breed in the ocean. All the mature females which we found, with one exception, were captured under these peculiar conditions; and we never failed to find them at this spot when the tide turned about sunset and the water was calm.

Owing to this singular limitation there are only a very few favourable evenings for procuring the eggs in a single season; and until the animals can be made to thrive and multiply in confinement, it must always remain an extremely difficult matter to procure the eggs in abundance.

Up to the present time our knowledge of the early stages of *Lucifer* has been extremely meagre.

In his report on the Crustacea of the United States Exploring Expedition, DANA described (p. 634) an organism under the name of *Erichthina demissa,* and figured it in plate 42, fig. 3. CLAUS ('Crustacean System,' p. 13) gives a figure of the same organism at a latter stage of development, and calls attention to the numerous features of resemblance to the *Protozoëa* stage of development of *Penæus.*

Only a few months before his death, the lamented WILLEMÖES-SUHM collected a number of specimens of *Erichthina* in the South Pacific, and, associated with them, a sufficient number of later stages to assure him that *Erichthina* is the larva of *Lucifer.* His account (" Preliminary Remarks on the Development of some Pelagic Decapods," by R. von WILLEMÖES-SUHM, Ph.D., Proc. Royal Soc., Dec. 9, 1875, p. 132–4) is very brief, and as it contains all that is known about the metamorphosis of this extremely interesting form, I quote it in full:—

" Very similar to that of *Sergestes* is the development of *Leucifer.* Here the earliest *Zoëa* of a species from the Western Pacific has got at first no eyes, then sessile ones came out, and the animal then presents the form which DANA has called *Erichthina demissa,* and which CLAUS suspected to be not a Stomatopod but a Schizopod larva. After the second moulting this *Erichthina* gets stalked eyes, and very long setæ on all its appendages, becoming a rather long, very delicate *Zoëa.* It now enters the *Amphion* stage, but never gets more than four pairs of pereiopods, and loses another pair of these when it moults for the youngest *Leucifer* stage, in which two pairs of pereiopods are absent.

"The next question after having found this out, was, of course, whether *Amphion, Sergestes,* and *Leucifer* leave the egg as a *Zoëa,* or whether there is a preceding *Nauplius* stage. My own impression is that in the two first-named genera this is not the case, as the youngest *Zoëas* which I caught had all the same size, and as none of them was without the large lateral stalked eyes. As for *Leucifer,* the question appears to me to be doubtful; for it is, from what I have seen, quite possible that my youngest *Zoëa,* which has only got a central eye, may be preceded by a *Nauplius.* Of course, the simplest thing would be to get the eggs; but there is the difficulty, for *Amphion* is caught very rarely, and has never been obtained at any other time but between 8 and 12 P.M., when it is extremely difficult by lamplight to find out the youngest stages. *Sergestes* larvæ are commoner, appearing also in the daytime, and *Leucifer* is sometimes caught in abundance. I hope, therefore, that I shall succeed in completing my researches about this question, especially as far as the two latter genera are concerned.

" H.M.S. 'Challenger,' Honolulu, Sandwich Islands.
"July 30, 1875."

As the sad death of this lamented naturalist, only a short time after, put an end to this as well as to his other researches in all departments of zoology, I take pleasure in stating that I have fortunately been able to complete his observations upon *Lucifer*, and to furnish a very perfect account of its entire metamorphosis, as well as a few important facts with reference to its development in the egg.

At the end of April, 1880, I found a single specimen of *Lucifer* with two eggs attached to one of its appendages, and I was led by the great importance and interest of the subject to make every effort to trace its life-history. For four months I met with no success whatever, but about the 1st of September I found a few advanced larvæ, and traced them to the adult, and I then succeeded in finding earlier stages and tracing them as far as the stages which I had previously found, but it was not until the last week of my season at the sea-shore that I succeeded in hatching the *Nauplius* from the egg, and the last gap in my series was bridged by a moult which occurred only a few hours before my departure.

As the result of my four months' efforts I can now state that I have seen the eggs of *Lucifer* pass out of the oviduct. I have seen the *Nauplius* embryo escape from the same egg which I had seen laid, and I have traced every moult from the *Nauplius* to the adult in isolated specimens. There is therefore no Crustacean with the metamorphosis of which we are more thoroughly acquainted than we now are with that of this extremely interesting genus.

Not only is it true, as WILLEMÖES-SUHM has pointed out, than DANA'S *Erichthina demissa* is a larval stage of *Lucifer*, but that DANA'S *Sceletina armata* is a later stage in the same series, while some of the forms which he includes in his genus *Furcilia* are also, in all probability, *Lucifer* larvæ.

The occurrence of a free *Nauplius* stage of development in the life-history of one of the higher Crustacea is a matter of such profound significance in the scientific discussion of the phenomena of embryology in general, that it can hardly be accepted without question so long as there is any possibility of error. Two of the observers who have testified to its occurrence have based their conclusions upon evidence which would be perfectly satisfactory in any ordinary case, but as they did not actually trace all the stages of development their statements do not stand the severe analysis which the importance of the case demands, and certain naturalists have therefore refused to give them unqualified acceptance.

The third observation was made so many years ago, and the larva is so briefly described, that it would not be safe to assume, in the absence of all corroborative evidence, that it is a *Nauplius* at all.

In December, 1838, DANA found in the harbour of Rio de Janeiro great numbers of specimens of a Schizopod, which he described ('United States Exploring Expedition during the Years 1838-1842,' under the command of CHARLES WILKES, U.S.N., vol. xiii., part i., p. 654) as *Macromysis gracilis*. In the brood-pouches of some of his specimens he found an abundance of eggs and developing embryos, several of which are shown in his plate 45, fig. 5. He made no careful study of their structure; his notice of them in the text is only a few words; and his figures are very small, and show the embryos in dorsal view, as seen under a very low magnifying power, but they are so much like FRITZ MÜLLER's figures, that we must acknowledge that the credit of the first discovery of a Malacostracan *Nauplius* belongs to DANA, and that up to the present time this is the only case in which a *Nauplius* has been traced to an egg which could be definitely identified as that of a specific adult Malacostracan, although his account is so imperfect that in itself it is certainly not sufficient to prove the existence of the *Nauplius* stage at all.

In 1861 FRITZ MÜLLER found, at Desterro, in Brazil, a single specimen of a *Nauplius* ("Die Verwandlung der Garneelen." Erster Beitrag von FRITZ MÜLLER, in Desterro, Arch. f. Naturgeschichte, 1863, p. 9), which he traced, through other specimens which were also collected in the ocean, to a form which he believed to change into the youngest *Zoëa* of a species of *Penæus.* The series of stages is so satisfactory that there is no reason for doubting the accuracy of his conclusion, but the chances for error, in the attempt to trace Crustacean development from isolated specimens, are so very great that the statement has not received unqualified acceptance.

The only other recorded observation of a Malacostracan *Nauplius* is not among the Decapods, but in the more embryonic Schizopods. These observations, which were made by METSCHNICKOFF, would tend to corroborate those by MÜLLER, but they are unfortunately open to the same criticism. He did not actually rear the larvæ and trace them to a specific adult, and although there would in ordinary cases be no doubt of the correctness of his conclusion, a careful analysis of his papers will show that there certainly is a possibility of error.

In the spring of 1868 he collected from the surface of the ocean at Messina a few early stages in the development of a Crustacean, which he believed to be *Euphausia mulleri* (CLAUS), and showed ("Ueber ein Larvenstadium von *Euphausia*" von EL. METSCHNICKOFF in Petersburg, Zeit. f. Wiss. Zool., xxix., 1869, p. 479, taf. xxxvi.) that it passes through a well-marked *Nauplius* stage, of which he gives three figures.

The following year, at Villafranca, he collected a good supply of young larvæ and floating eggs in advanced stages of development, and was thus enabled to supplement his first paper by a second ("Ueber die Naupliuszustände von *Euphausia*," von ELIAS METSCHNICKOFF, Zeit. f. Wiss. Zool., xxi., 1870; p. 380, taf. xxxiv.) in which he gives a minute account of the *Nauplius* from the time it leaves the egg until it changes into a form somewhat similar to the youngest stage of *Euphausia*, which had been previously described by CLAUS ("Ueber einige Schizopoden und niedere Malacostraken Messinas," von Prof. Dr. C. CLAUS, Zeit. f. Wiss. Zool., xiii., 1863, p. 422). CLAUS had supposed this to be the stage in which the larva leaves the egg, and he says (p. 450), "Diese Larve bin ich geneigt für die jüngste aller freieren Entwickelungsformen der *Euphausia* anzusehen." He subsequently learned, however ("Untersuchungen zur Erforschung der Genealogischen Grundlage des Crustacean-Systems," p. 9), that he had been in error, since he afterwards found, in an Atlantic and also in a Mediterranean species, an earlier *Protozoëa* stage, which changed into the *Zoëa* described in his first paper. It therefore follows that METSCHNICKOFF studied something else, or that he was in error in believing that he had traced his *Nauplius* directly to what CLAUS has shown to be a somewhat late stage in the development of *Euphausia*. METSCHNICKOFF's only reason for believing that his *Nauplius* is a young *Euphausia* is its resemblance to CLAUS's larva, and as there is certainly an error here, we are not justified in giving unqualified acceptance to his statement that it is an *Euphausia* larva. It seems very probable, indeed, that this is the case, but in the absence of the direct evidence which could only be afforded by actually tracing it back to an *Euphausia* egg, or forwards to the adult *Euphausia*, I do not think that the existence of a Malacostracan *Nauplius* can be said to be established by these observations, for they do not stand the severe test which is demanded by their unusual importance, and I think the facts justify the statement that, up to the present time, there has been no unquestionable evidence of the occurrence of such a stage of development in the higher Crustacea.

The present series of observations is complete at both ends, and I have not relied upon surface-collecting to fill a single gap, but have traced every stage in isolated captive specimens, and the possibility of error seems to be entirely out of the question.

The close resemblance between the *Nauplius* of *Lucifer*, and MÜLLER's and METSCHNICKOFF's larvæ, renders it almost certain that they also are Malacostracan larvæ, but before this corroborative evidence was furnished, it was certainly quite possible, although hardly reasonable, to doubt whether this was true of either of them.

II.—THE SEGMENTATION OF THE EGG, AND FORMATION OF THE FOOD-YOLK AND PRIMITIVE DIGESTIVE CAVITY.

Unusual difficulties attend the study of the early stages in the embryology of *Lucifer*, and the observations which I have been able to make are incomplete, and leave many gaps to be filled and many interesting points to be decided by future investigations; but the facts which I have made out are so novel, so different from all that was previously known of the early stages of Arthropod development, and they throw so much light upon the relation of the peculiar and greatly modified form of segmentation characteristic of the group to the less modified form of segmentation presented by the more normal eggs of other animals, that it seems best to give my results in their present incomplete state.

I am the more willing to do this, because the peculiar difficulties of the subject leave little hope for the attainment of more complete results in the future.

The eggs are so loosely attached to the appendages of the female that they are broken off by the slightest roughness of handling, and it is very difficult to obtain them by collecting the egg-bearing females. Even when great numbers of mature specimens are captured in the breeding season, with the greatest care and delicacy, very few of them, much less than 1 per cent., are found to have eggs attached to their limbs when the collection is examined.

If the mature animals could be induced to thrive and multiply in confinement, there would be no difficulty in obtaining a sufficient supply of eggs, but until this can be done it must be extremely difficult to procure them in sufficient numbers for exhaustive study.

During the early stages the eggs are so delicate that they are soon destroyed by the confinement and compression to which it is necessary to subject them while they are under examination, and it is therefore impossible to watch very many stages in a single egg.

When we add to this that the eggs are laid about 9 o'clock in the evening, and must be studied between this time and daylight, after several hours of laborious collecting, by eyes that have been already severely taxed with looking over the collections and picking out the transparent and almost invisible adults by an artificial light, and examining each one of them with a lens to find those which carry eggs, the difficulty of the subject will be appreciated.

The eggs are spherical, transparent, and they contain extremely little food-material. This is uniformly distributed over the whole egg in minute globules, which have nearly the same colour and refractive index as the surrounding protoplasm.

The egg undergoes total regular segmentation, and a true segmentation cavity occupies the place filled by the large central yolk-mass in the eggs of other Arthropods.

It first divides into two equal portions (Plate 1, fig. 1); then, by a cleavage at right angles to the first, into four (fig. 5); then into eight (fig. 8); then into sixteen (fig. 10); and so on.

At the stage shown in fig. 10 the inner ends of the sixteen spherules are seen to be separated from each other by a central space, the segmentation cavity, which persists, and is shown at later stages in figs. 11, 13, 15, 16, 17, 19, and 20, at *b*.

In fig. 10 the egg will be seen to be spherical, and all the segments have their broad ends at the surface; but in the next stage one pole of the egg becomes a little flattened, and in an optical section the spherule (*c*), which occupies the centre of the flattened area, is seen to have its broad end nearest the centre of the egg.

Most of the food-material has meanwhile disappeared from the other spherules, which are now quite transparent, while the spherule (*c*) still contains as much as ever, but apparently no more than there was contained at an earlier stage in an equal area of any part of the egg. In an optical section of the same egg, in a plane at right angles to that of fig. 11, the spherule (*c*) shows a trace of a fissure, which a little later divides it into two (see fig. 12, *c*).

Plate 2, fig. 13, is an optical section, like the one given in fig. 11, of a somewhat older egg; and fig. 14 an optical section of the same egg at right angles to fig. 13. The outline is a little more flattened on one side than it is in fig. 11, and the spherule (*c*) is completely divided by a radial fissure into two, and these project into the segmentation cavity (*b*) a little more than they did before.

In fig. 15 the flattening has become a deep pit (*d*), and the spherules (*c*) have been pushed quite into the segmentation cavity, and the adjacent cells have begun to move in the same direction. This change is more marked in fig. 16; and in fig. 17 the egg consists of a double wall of cells, the ectoderm and the endoderm, surrounding a primitive digestive cavity (*d*), and separated from each other by the segmentation cavity (*b*), in which the two cells (*c*) are situated. Each of these also shows traces of a division into two.

These changes are more marked in fig. 19; and in fig. 20 the opening of the primitive digestive cavity is much reduced in size, and the cavity itself does not lie exactly in the axis of the egg, but at one side of it.

A more minute examination of the segmentation brings out a number of interesting points; one of them is the rhythmical character of the process, which is not a continuous uniform change, but a series of stages of activity, separated from each other by periods of rest.

The egg shown in Plate 1, fig. 1, was laid about 10 o'clock P.M., and about 10.35 it was in the condition which is represented in the figure. As I had not been watching it I did not observe the first division, and when first seen it was in the resting condition, and the two spherules were not sharply defined, but pressed together.

During the next fifteen minutes no external change was visible, and the drawing was made at 10.50 P.M. It then entered upon the second period of segmenting activity, and in five minutes the two spherules were well defined, as shown in fig. 2; and in five minutes more (fig. 3) one of them showed traces of division into two. In ten minutes more (fig. 4) this division was completed, and traces of a similar change had made their appearance in the other spherule, which was also perfectly divided into two at the end of five minutes more (fig. 5). This stage ended the second period of activity, which was twenty-five minutes long.

During the whole of this time the egg showed gradual and uniform change, which was sufficiently rapid to be distinctly visible. Although four so-called stages are figured, there was no division into stages, but a continuous change without interruption.

The four spherules now began to flatten down, and in five minutes the egg was in the condition which is shown in fig. 6, and it then remained without any external change for more than ten minutes. The second period of rest, measured from the time when the four spherules began to shrink together to the time when they began to swell out and enter upon the third period of active segmentation, was therefore more than fifteen minutes long.

At 11.40 the four spherules were once more sharply defined (fig. 7), and changes went on uniformly until, at 12.15 A.M., each was perfectly divided into two, as shown in fig. 8, which marks the end of the third period of activity, thirty-five minutes long.

I was not able to watch this egg pass into the next resting stage, as it had been so long under observation (1 hour 45 minutes) that its development was arrested at this point; but another egg in this stage of development was seen to pass into the resting condition, as shown in fig. 9, and it then remained quiet for about fifteen minutes, showing no external indications of change during this time.

At the end of the third period of rest the spherules again became prominent, so that the outline of the egg was exactly like that of fig. 8, and the egg entered upon the fourth period of activity, soon dividing into sixteen spherules (fig. 10), arranged around a segmentation cavity.

In about twenty-five minutes from the beginning of this period of activity the spherules began to flatten down once more, and the egg passed into the fourth resting stage, but it was not observed beyond this point.

The alternation of activity with rest was observed at much later stages, but after the gastrula invagination makes its appearance the cells of the endodermic portion of the egg do not undergo active change at the same time with those of the ectoderm, and the egg has one set of periods of activity for each layer. As development goes on the periods of rest grow longer and the periods of activity shorter, and the spherules do not flatten down while at rest.

The egg which is shown in optical longitudinal section in fig. 16 was in the field of the microscope for nearly twenty minutes, while I was examining another specimen. An occasional look at it showed that it was not changing, but at the end of this time I noticed that the outer ends of the ectoderm cells directly opposite the orifice of invagination were notched, as is shown in the figure. Activity spread in all directions from this point, and in less than five minutes all the cells were notched, and those nearest the centre of the area of activity were perfectly divided into halves. In about five minutes more all the ectodermal cells had divided, and this layer had the appearance shown in fig. 17—which, however, was drawn from another specimen.

This last egg remained in the condition shown in the drawing for fifteen minutes from the time it was first observed, and a movement of the appendage to which it was fastened caused it to roll over and present its formative pole for examination before the beginning of the next period of activity, which is shown in surface view in fig. 18. The manner of division was simply a repetition of that which has just been described.

The cells nearest the centre of the formative area became notched, and then divided into halves; and the activity gradually spread over the egg in all directions, until, in a few minutes, all the cells which were visible were at some stage of division.

The rapidity and uniformity with which this change spread over the egg rendered it an extremely interesting and impressive sight, and I know of no other case in which segmentation is so perfectly regular at such an advanced stage of development.

The activity did not affect the endoderm cells in either of these cases, but at a later stage (fig. 20) they were seen to be in an active segmenting condition at a time when the ectoderm cells were at rest. I was not able to keep this egg alive long enough to watch the completion of the process, for it had been under the microscope for some time before the stage shown in the figure was reached; but the division of the endoderm cells appears to go on much more slowly than that of the ectoderm cells.

This phenomenon, the alternation of periods of rest with the periods of active segmentation, does not seem to have received from embryologists the attention which it deserves. A number of observers have pointed out that in many animals, among the Mollusca especially, the distinctness of the spherules becomes more or less completely obscured after each division, and that this state persists until just before the next division, when the spherules swell out and again become conspicuous. The change of form does not seem to be at all general, and in most accounts of segmentation nothing of the kind is recorded.

I believe that it is a secondary phenomenon, and that the essential thing is the alternation of rest with activity; and I am confident that careful time records of segmentation will show that this occurs in nearly every case, sometimes with and sometimes without the accompanying change of form.

I have observed it in *Physa*, *Limnæus*, and *Planorbis*, where segmentation is total and nearly regular; in the Oyster, where the egg has a rudimentary food-yolk and segmentation is irregular; in a bony fish with a large food-yolk and a discoidal segmentation; and in *Lucifer*. Other investigators working under my guidance have observed it in *Amblystoma* and in oligochætous and polychætous Annelids. These are all the cases in which I have been able to test the matter since my attention has been attracted to the subject; and as the alternation was found to occur in every case, although the animals are so widely separated and present such diverse modes of segmentation, I feel justified in assuming that the phenomenon is general, and will be found in all eggs which can be properly examined by watching and timing them while segmentation is going on.

The cause of rhythmical physiological change is an extremely interesting question; and as the segmenting egg exhibits the phenomenon in the greatest possible simplicity, it would seem to be a peculiarly favourable subject for investigation.

The phenomena which have been described seem to show that segmentation is not due to the action of any purely molecular force, like polarity, but is essentially a vital activity, and in a paper on the embryology of the fresh-water *Pulmonates* ('Studies from the Biological Laboratory of the Johns Hopkins University,' vol. i., part ii.) I have ventured the following explanation.

During the period of segmentation the protoplasm of the whole egg (of *Physa*) gradually becomes more and more transparent, on account of the gradual disappearance of the granular food-material which it contains, and the rhythmical character of the process of segmentation would seem to admit of a simple explanation on the supposition that the physical properties of the protoplasm offer a resistance which must be overcome before the force which is set free by the assimilation and reduction of the food-material can exert itself to bring about the active changes of segmentation. During a period of rest the process of digestion and assimilation accumulates a store of energy which, at length, becomes sufficient to overcome this resistance, and to initiate a period of activity which lasts until the whole of this reserve of force has been expended in the rearrangement of the protoplasm. The physical properties of the protoplasm now reassert themselves, and tend to reduce the whole egg as nearly as possible to a spherical form once more, and the egg then remains inactive until the supply of energy again becomes great enough to overcome the resistance.

If this is the true explanation we should expect to find the alternation of rest and activity much more general than the change of form, for the degree of consistency of the protoplasm or the amount or character of the food-material, or the way in which it is distributed through the egg, may prevent the second set of changes from showing themselves. This is precisely what we do find, and in the bony fishes, where the large food-yolk would prevent any marked change of form, we find the first set of changes well marked, but with no trace of the second set.

Leaving this subject for the present, I wish to say a few words about another interesting phase of the early stages of *Lucifer*. We cannot fail to be impressed by the very remarkable departure from ordinary Arthropod segmentation, nor can we overlook the fact that in all the points of difference from the eggs of allied forms, the eggs of *Lucifer* show a most suggestive resemblance to the ordinary unspecialized ova of other Metazoa.

In an ordinary Arthropodan egg we have, as the outcome of the process of segmentation, a central mass of food-yolk, which may or may not be divided into segmentation products, and which completely fills the segmentation cavity; and an outer investing layer of blastoderm cells; that is, the egg undergoes a centrolycethal segmentation.*

In most Crustacea the early stages of segmentation are regular, and apparently total, but the lines of cleavage do not pass entirely through the egg, and the spherules are united to each other by a central mass of food-yolk. When segmentation is somewhat advanced the products of segmentation become more or less pyramidal, with the bases of the pyramids at the surface, and their apices fused together at the centre of the egg. The outer ends of the pyramids then become transparent and separate off as a blastoderm, while the inner portions usually fuse together, more or less perfectly, to form a central food-yolk, which fills the space which in ordinary eggs constitutes the segmentation cavity. A small portion of the blastoderm then becomes invaginated to form the primitive digestion cavity, and the remainder becomes the ectoderm.

The centrolycethal type of segmentation presents great variations in the different groups of Arthropods, but in nearly all cases its peculiarities are so well marked

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* The whole subject of segmentation has been so ably and exhaustively reviewed by BALFOUR in his recent work on 'Comparative Embryology,' that it does not seem necessary to burden this paper with a long list of references to the literature of Arthropod segmentation, or to enter into an exposition of the present state of our knowledge of the subject. All the essential facts and opinions may be found on pages 79–99, 317–379, and 425–433 of vol. i. of the 'Comparative Embryology.'

that it is difficult to trace any resemblance to the various forms of segmentation which occur in other groups of animals. In *Lucifer* the case is reversed, and we have a type of segmentation which is obviously similar to that of the Echinoderms, Annelids, Molluscs, Tunicates, Vertebrates, &c., but is less obviously related to that of the eggs of closely allied forms. The resemblance to what may be called "normal" segmentation is so plain that it need not be dwelt upon, but the relation between the egg of *Lucifer* and an ordinary centrolycethal egg is by no means clear.

It seems probable, however, that since the food-material which has not been assimilated becomes centralised, after segmentation is somewhat advanced, in the single spherule *c*, of fig. 11, this spherule must correspond to one of the yolk-pyramids of an ordinary Crustacean egg. This then divides, by radial fission, into two portions (fig. 13, *c*), and it seems probable that the food-material then becomes restricted to their central ends, while the outer protoplasmic ends separate off as a pair of blastoderm cells (fig. 15), thus leaving the two masses of food-yolk (*c*) inside the segmentation cavity. While I was investigating the subject I regarded the spherule *c*, of fig. 11, as a primary mesoblast, which became pushed into the segmentation cavity, and then divided up to form the mesoderm; and I expressed this view without comment in a preliminary abstract of the subject ("Embryology and Metamorphosis of the Sergestidæ," Zoologischer Anzeiger, iii., p. 563). In most cases where the origin of the mesoderm has been most carefully studied, it originates by the separation of the inner ends of the cells which are to give rise to the endoderm, either before or during or after the invagination takes place; the mode of origin of these spherules in *Lucifer* and their position in the egg agree with what we should expect if they belong to the mesoderm, but the great quantity of food-material which they contain would hardly be looked for in this case, and favours the view that they are yolk-pyramids rather than mesoblasts.

As I examined no eggs between fig. 20 and fig. 21, the later history is uncertain, but a reference to figs. 21, 22, 23, and 24, which are about twenty hours later than fig. 20, shows that the region of the digestive tract of the *Nauplius* is marked by the presence of a number of large polygonal masses of what appears to be food-yolk, and it seems probable that these are the derivatives of the spherules *c*, of fig. 20. I was not able to actually witness the change from fig. 11 to fig. 15, and cannot state with absolute certainty that the spherules *c* divide into a central and a peripheral portion. Fig. 15 seems to indicate that this is the case, but in the absence of direct observation of the change, it is possible that the two cells which in fig. 15 lie below the cells *c*, are the ones which were at its sides in fig. 11.

If each of the cells *c* gives rise to a blastoderm cell, we should expect to find two more cells in fig. 15 than in fig. 18, but the number is the same. This is hardly a safe guide, however, for while the drawings are careful copies from Nature, they are not from the same egg, and the cells are so wedged together that vertical sections in different planes would not intersect the same number in all cases, and there may have been two more in fig. 15 than in fig. 13.

I think, then, that the facts indicate that *c* of fig. 11 is a yolk-pyramid, rather than a primary mesoblast, and that after it divides into two, as in fig. 13, each part gives rise to a central portion *c*, and a peripheral endoderm cell.

If we accept this view and regard the cell *c* as a yolk-pyramid, two views as to the relationship between the egg of *Lucifer* and an ordinary Crustacean egg at once suggest themselves.

We may hold that *Lucifer* presents the primitive or ancestral form of segmentation, of which centrolycethal segmentation is a secondary modification. In this case we may suppose that as the supply of food-material gradually increased, new food-bearing cells or yolk-pyramids were added until all the cells were included, and the segmentation cavity was entirely filled and obliterated by them.

According to the other view, we may hold that the segmentation of the *Lucifer* egg is a secondary modification, which has been brought about by the gradual reduction of the amount of food-material, and its restriction, at last, to a single one of the cells of the segmenting egg.

There does not seem to be much difficulty in deciding which of these views is most satisfactory and probable. *Lucifer* is undoubtedly a very primitive Malacostracan, but it can hardly be regarded as a primitive Crustacean; and the occurrence of perfectly centrolycethic segmentation in the Copopods, Phyllopods, Amphipods, and Isopods, as well as in the Decapods—forms below as well as forms above *Lucifer*—forbids us to believe that the egg of *Lucifer* is ancestral, or the unmodified descendant of an ancestral type of egg; and we must therefore believe that the egg of *Lucifer* has been simplified by the loss of the greater part of its food-yolk.

A change of this kind is not without a parallel, and I have shown ("The Acquisition and Loss of a Food-Yolk in Molluscan Eggs," 'Studies from the Biological Laboratory of the Johns Hopkins University,' vol. i., part iv.) that the resemblance between the segmenting egg of the Oyster and a Molluscan egg with a food-yolk can only be explained by the supposition that the Lamellibranchs have inherited a rudimentary food-yolk which was functional at some past time, and that the assumption gives an explanation of all the peculiarities of oyster segmentation.

If we accept this view, and regard the egg of *Lucifer* as simplified by secondary change, it is extremely instructive to note that the loss of a food-yolk has brought it back to a type of segmentation which is directly comparable with that of ordinary Metazoan eggs, and we must therefore believe that a segmentation cavity is potentially present in all centrolycethic eggs, or else that the segmentation cavity of the egg of *Lucifer* is not homologous with that of ordinary eggs.

III.—GENERAL ACCOUNT OF THE METAMORPHOSIS OF LUCIFER.

The most instructive method of studying the metamorphosis of *Lucifer* is to trace each part of the body through the series of changes which it undergoes from its first appearance until it assumes the adult form; but as this method of comparing the successive stages in the development of each organ necessarily involves references to other organs, it seems best to give first a general account of the whole structure of the larva at each stage of development, and afterwards to go over the same ground more rapidly in a different way, and to trace the history of each appendage.

The egg Nauplius.

About thirty hours after oviposition the eye spot and appendages of the *Nauplius* became visible inside the egg-shell, as shown in a ventral view in Plate 2, fig. 21, and in a dorsal view in fig. 22. If the egg-shell is torn at this stage the embryo escapes, and swims about quite vigorously for a short time, but soon dies. The various parts of the body are much better shown in the swimming embryo than while it is contained in the egg, and I therefore give, for comparison with figs. 21 and 22, a dorsal view (fig. 23) and a ventral view (fig. 24) of an embryo which has thus been set free.

Fig. 23 shows an embryo of exactly the same age as those in figs. 21 and 22, while fig. 24 was drawn from an embryo a few hours older. The difference in the outline of the body is not due to this difference in age, however, but to a slight change in the point of view. In all four figures the letter *e* marks the anterior end of the body, and fig. 22 is a view directly opposite to fig. 21. Fig. 23 is in the same position as fig. 22, but the embryo shown in fig. 24 was in such a position that more of the anterior surface and less of the posterior surface was visible than in the other figures.

On the median line of the ventral surface the labrum (figs. 21 and 24, *L*) is very conspicuous at the anterior end of the body, and behind it there is a double row of four pairs of bud-like eminences, arranged in a longitudinal series. The first pair (figs. 21 and 24, *g*) are much larger than the others, and the depression which separates them on the median line is less marked than it is in the three pairs which lie behind. It is rather difficult to decide with certainty what this pair of buds becomes, but in the larva which METSCHNICKOFF studied the changes were more gradual than they are in *Lucifer*, and he was therefore able to trace their history more satisfactorily, and to show that they become the metastoma. Their position with reference to other parts indicates that they have the same history here, and that the other three pairs of buds are the first and second maxillæ and the first pair of maxillipeds (*Mx.* 1, *Mx.* 2, and *Mp.* 1).

Three pairs of much larger appendages are folded down on to the sides of the body, within the egg; and when the embryo is set free they are seen to be the first antennæ

(*A*), the second antennæ (*An*), and the mandibles (*M*). They are not divided into joints or rings, although the second antennæ and the mandibles are biramous, and consist of a basal portion or protopodite, an expodite, and an endopodite. All three pairs have hairs projecting from their tips, and these lengthen considerably within a few minutes after the embryo is freed from the egg. The first antennæ are nearly as long as the second, and both pairs, as well as the mandibles, are organs of locomotion, to row the animal through the water. The motions of the larva are very erratic and violent, and consist of a series of quick leaps produced by vigorous backward strokes of the appendages.

The outline of the body will be understood by a reference to the figures. When the second maxillæ are in the centre of the field of view, as in fig. 21, the outline is pear-shaped, with the broad end of the pear at the posterior end of the body; but when the metastoma is in the centre this is reversed, and the broad end is in front. This difference is due to the fact that the dorsal region is much wider than the labrum and series of buds, which together form a ridge along the ventral surface.

In a dorsal view the simple eye (*Oc*) is seen as a black spot on the middle line, near the anterior end of the body. It did not show any traces of a division into halves at any stage of development which was observed.

The ocellus lies upon a large rounded granular body, which is imperfectly divided into halves by a notch upon its posterior margin. This body consists of the fused cerebral ganglia.

The dorsal portion of the posterior region of the body is swollen and rounded, as shown in figs. 21 and 23; and near its lateral margins there are a pair of small, but very conspicuous, dark pigment-spots (*i*), which might easily be mistaken at this stage for ocelli, since they have almost exactly the same size and colour. These two pigment-spots are very conspicuous during all the early stages of the metamorphosis, and their position during the later stages (figs. 25, 26, 27, 34, 35, and 47, *p*) shows that the portion of the *Nauplius* body which bears them becomes the thoracic, not the abdominal, region of the adult.

In the interior of the enlarged posterior portion of the body there is a huge mass of polygonal highly-refractive bodies, which appear to constitute a food-yolk, and which surround the digestive tract of the embryo. I have already given my reasons for believing that those bodies are derived from the spherule which becomes pushed into the segmentation cavity during the early stages of development. If this is their origin they must increase in size between the stage shown in fig. 20 and that shown in fig. 21. This is not at all an unusual occurrence, and in the fresh-water Pulmonates the yolk-spherules which surround the digestive tract continue to grow until a very advanced stage of development. I found so few eggs at this stage that I was afraid to sacrifice any of them by attempting to study their internal structure under pressure, and I am not able to give an account of the digestive tract or of the other internal organs.

When the embryo is set free from the egg it is seen to be inclosed by a delicate cuticle, which is shown, around the antennæ, in figs. 23 and 24. It is soon stripped off by the vigorous movements of the larva, and in fig. 24 it has been torn from all the appendages except the first antennæ (*A*).

In a dorsal view a number of muscular fibres are seen to extend outwards and forwards from the median line of the body to the basal joints of the antennæ.

The posterior end of the body is not notched, the anus is absent, and there is no trace of the telson or of the carapace.

The first free Nauplius *stage.*

About thirty-six hours after oviposition the larva escapes from the egg as a *Nauplius*, $\frac{8}{1000}$ inch long, which is shown in side view in Plate 3, fig. 25. There is now no difficulty in keeping it alive and rearing it, and it swims very actively by vigorous strokes of its two pairs of antennæ. Its movements are very characteristic, and much like those of a Copepod or Cirrhiped *Nauplius*.

The most important differences between it and the egg *Nauplius* are the segmentation of the locomotor appendages, the lengthening of their hairs, the increased size and dendritic form of the pigment-spots (*p*), and the appearance of the telson (*Tl*), as a projecting fold furnished with two pairs of short spines or hairs, in the ventral surface of the posterior end of the body.

As regards the more minute structure of the appendages, the first antennæ (fig. 25, *A*) are five-jointed, and the hairs, which are more than half as long as the limb, are borne on the terminal joint.

The second antenna consists of a two-jointed basal portion or protopodite which carries two rami, one of which (fig. 25 *ex*), is obscurely divided into three nearly equal joints, while the other (fig. 25, *en*), is divided into eight very distinctly marked joints. Both at this stage and later the appendage possesses considerable power of rotation, and sometimes the branch *ex*, and sometimes the branch *en*, is on the outer surface. It is therefore very difficult to decide from an examination of this appendage alone which branch is the exopodite and which the endopodite ; but, as I shall show further on, a comparison with other appendages at a later stage indicates that the eight-jointed ramus is the endopodite, although the limb is frequently, and perhaps generally, carried in a position which brings this branch on to the outside. At this stage the locomotor hairs of both branches are confined to the tips of the terminal joints. The first and second joints of the endopodite are quite short, while the other six are longer and nearly equal in length.

The mandible consists of a short unjointed basal segment, which carries a one-jointed endopodite, and an obscurely three-jointed exopodite. Each branch carries three hairs, which are somewhat longer than the limb, and the entire length of the appendage, including the hairs, is about equal to the length of the first or second antenna, without its hairs. There are no cutting blades or hooks upon the basal joints of either pair of antennæ or the mandibles.

The labrum (L) is somewhat larger and more prominent than it was at the stage before, and the anus is still absent.

The second free Nauplius *stage or meta*-Nauplius.

In about twelve or fourteen hours the *Nauplius* sheds its skin and assumes the form shown in Plate 3, fig. 26. From the prominence of the region of the hind body, and the presence of a carapace, Claus has distinguished this stage of development, in allied forms, by the name of meta-*Nauplius.*

I did not actually witness the change, and am not sure of the exact length of the first free *Nauplius* stage, but it is not more than eighteen, and probably no more than twelve hours long. A *Nauplius* which had hatched from the egg some time during the latter part of Monday night was placed, alone, in a watch-glass of sea-water, and changed into the one from which fig. 26 was drawn before 9 P.M. on Tuesday evening.

The differences between this and the preceding stage are sufficiently great to attract the attention at first sight. The length, as measured from the ocellus to the posterior end of the body, has increased from $\frac{8}{1000}$ inch to $\frac{9}{1000}$ inch. The labrum (L) is longer and more prominent. The first antennæ (A) are unjointed, and the joints of the second antennæ (An) and mandibles (M) are almost absent.

The hairs at the tips of the endopodites of the second antennæ and mandibles (en) are irregularly plumose, and a long slender slightly curved hair is carried by each of the larger joints of the endopodite of the second antennæ.

On the inner posterior edge of the basal joint of the mandible, a short stout curved hook or blade has made its appearance. The four pairs of buds on the ventral surface, posterior to the labrum, are in the same condition as before, but the telson (T) is quite prominent, notched or forked, and furnished with two pairs of short stout spines, the inner pair being much longer than the outer. A well-marked fold (c) of the surface of the body now marks the posterior and the lateral edges of the carapace, but this line is not continued on to the anterior end of the body, and the posterior edge is not yet raised or separated from the hind body as it is, according to Metschinckoff, in the last *Nauplius* stage of *Euphausia.*

The pigment-spots (p) are drawn out in such a way as to surround a large rectangular area, at the posterior end of the carapace, and in the region where the heart is placed at the next stage.

The digestive tract is now visible in a side view. The œsophagus (α) runs upwards and forwards from the mouth, situated under the overhanging tip of the labrum, and then bends backwards and upwards to open into the floor of the stomach (s) ; the side walls and top of the stomach could be made out without difficulty, but I was not able to decide whether its ventral wall is complete or not. It is divided by a fold or flap in its dorsal wall into a small rounded anterior chamber, into which the œsophagus opens, and a longer posterior chamber, with its dorsal wall very thick, which gives rise at its posterior end to the intestine (*i*). The greater part of the anterior chamber lies in front of the œsophageal opening. On each side of the stomach there is a group of polygonal yolk-cells (*l*), which are by no means as conspicuous as they were at an earlier stage. The intestine is small, with thin walls, and it follows the dorsal curvature of the body to the anus, which was visible in a ventral view just in front of the spines of the telson, at the point marked (*a*) in fig. 26. The cerebral ganglia (*ga*), and the ocellus (*oc*), are still visible, and underneath the stomach there is an elongated granular body (*n*), obscurely divided into segments, which is, without doubt, the rudimentary ventral nervous system.

As it was necessary to keep this larva alive I did not dare to use much pressure whilst examining it, and was therefore unable to make a very thorough study of its internal structure.

The first Protozoëa *stage.*

On Tuesday evening, September 28th, at 9.30 P.M., the *Nauplius* which has just been described was placed alone in a watch-glass of sea-water, and at 9 A.M. on Wednesday, the 29th, it had changed into the larva which is shown in dorsal view in Plate 3, fig. 27. The number of segments and appendages of this larva and its general form and proportions are like those of the *Euphausia*, *Penæus*, and *Sergestes* larvæ at the stage of development which CLAUS has proposed to call a *Protozoëa* ('Crustacean System,' p. 2). The precise time when the change took place could not be learned, but there is reason to believe that it was not much later than the middle of the night. On September 14th I obtained, by dipping with a surface-net, a *Protozoëa*, which I studied and drew. It was of exactly the same size ($\frac{20}{1000}$ of an inch measured from the tip of the rostrum to the bases of the spines of the telson) as the one which moulted from the *Nauplius*, and it agreed with this in every respect except that the free segments of the hind body, shown in fig. 27, were wanting. It hardly seems probable that there are two stages of exactly the same size between 9.30 P.M. and 9 A.M., and it is much more probable that the body segments do not become distinct until some time after the moult, and as the larva had them at 9 A.M., I infer that it was nearer the end than the beginning of the first *Protozoëa* stage, and that the change had taken place some hours before I examined it.

CLAUS is inclined to believe that the difference between FRITZ MÜLLER's last figure of the *Nauplius* of *Penæus* and his first figure of the *Protozoëa* is so great that there must be a gap in the series of observations. The isolated *Nauplius* of *Lucifer* passes through quite as great a change in twelve hours, and its length increases from $\frac{9}{1000}$ to $\frac{20}{1000}$, or more than 100 per cent., and there does not seem to be any necessity for supposing that FRITZ MÜLLER has missed a stage in order to account for the change in his larva.

In the case of *Lucifer* the actual increase in size is not very great, but the carapace becomes folded out over the body, and the thick posterior portion of the body of the *Nauplius* becomes pulled out into the long free movable hind body of the *Protozoëa*, so that the length is more than doubled, while the vertical thickness of the body is correspondingly reduced. The shape of the larva when seen from one side will be understood by a reference to Plate 4, fig. 35, for although this figure was drawn from an older larva, it correctly represents a side view of fig. 27 in all essentials.

The most marked differences between the meta-*Nauplius* of *Lucifer* and the *Protozoëa* are due to the development of the carapace and the hind body. The carapace (fig. 27) is horse-shoe shaped, with smooth lateral and posterior edges, and it forms about one-half of the total length of the body. On the median line of the anterior edge it is drawn out into a long rostrum (*R*), at the base of which are the cerebral ganglia (*ga*) and the ocellus (*Oc*). On the median line of the posterior edge of the dorsal surface there is a shorter dorsal spine (*ds*), and at the outer angles of the posterior edge a pair of lateral spines (*ls*), which are a little longer than the dorsal one. The side view (fig. 35) shows that the sides of the carapace have folded down on to the sides of the body, and all the appendages, except the antennæ, are almost completely covered by it. The appendages are so nearly alike in this and the next stage that it will be most convenient to describe them together.

The stomach (*s*) is now divided into a pair of anterior or cephalic, and a pair of posterior or hepatic lobes, and between the cephalic lobes a number of muscular fibres run upwards and forwards from the œsophagus to be attached (at *m*) to the carapace. The intestine is small and straight (*i*), but it is not of uniform character, and is divided into a series of small enlargements separated from each other by constricted portions.

The last of these enlargements is much more constant than the others, and its walls are attached to the integument of the abdomen by a number of small muscles.

It exhibits regular pulsations, which seem to draw water into and out of the anus (*a*), which is on the ventral surface of the telson.

The heart (*h*) is compact, short, situated near the posterior edge of the carapace, and it gives rise to a single median and two lateral anterior arteries.

The hind body is about as long as the carapace, and it is divided into four somites and a long unsegmented region (*abd*). The study of the appendages shows that the four somites are those which carry the third pair of maxillipeds (*Mp.* 3), and the first, second, and third thoracic somites (*T* 1, *T* 2, and *T* 3). There are no traces of appendages on any of them. The end of the unsegmented region of the hind body forms a well-marked flattened telson (*T*), which is slightly notched on the median line, and carries four pairs of stout spines, and one pair of very small ones. The small ones are nearest the median line; the third pair are the longest and largest, and the fifth pair spring from the edges of the telson, some distance from the end.

The second Protozoëa *stage.*

As my season's work at the sea-shore ended the day the *Nauplius* shown in fig. 26 turned into the *Protozoëa* shown in fig. 27, I was not able to trace the development of that specimen; but on September 14th I had captured and drawn a larva in the same stage, and this moulted, while isolated in a watch-glass, into the second *Protozoëa* which is shown from above in Plate 3, fig. 34, and from the right side in Plate 4, fig. 35.

This larva measures $\frac{27}{1000}$ inch from the tip of the rostrum to the fork of the telson. The appendages are like those of the first *Zoëa* in number and structure, but there is a well-marked difference in the shape of the body. The carapace is somewhat elongated, its anterior edge is less perfectly rounded than before, and a pigment-spot (fig. 34, *E*) represents the future compound eye.

The pouches of the stomach (*s*) are much more conspicuous than before, and the œsophagus (fig. 34, *œ*) is visible in a dorsal view, between its anterior or cephalic lobes. The four somites of the hind body (*Mp.* 3, *T* 1, *T* 2, and *T* 3) have become short, but there is, as yet, no trace of their appendages. The unsegmented portion of the abdomen (*abd*) has increased in length, as have also the spines of the telson (*T*). The two pairs of antennæ have substantially the same form that they had during the *Nauplius* stage, and they are still the chief locomotor organs. The larva swims by jerks like a *Nauplius* or a Copepod.

The appendages at this as well as at the preceding stage are as follows (see Plate 4, fig. 35): the long uniramous first antennæ (*A*); the biramous second antennæ (*An.*); the cutting mandibles (*M*); the biramous first and second maxillæ (*Mx.* 1, *Mx.* 2); and two pairs of biramous maxillipeds (*Mp.* 1, *Mp.* 2).

The first antennæ consist at both stages (figs. 27, 34, and 35, *A*) of a long cylindrical basal joint which carries a few short hairs, and a short pointed terminal joint or flagellum, which ends in two long rather thick sensory hairs.

The second antennæ (figs. 27, 34, and 35, *An*; and fig. 36) are the chief locomotor organs, and are made up of a short stout two-jointed basal portion, a longer unjointed exopodite (*ex*), with four long terminal swimming hairs, and a longer endopodite (*en*), which is made up of two short proximal rings, and a series of six longer joints, each of which carries one, and the terminal one four, long slender swimming hairs.

Underneath the rostrum (fig. 35, *R*) there is a little elevation upon which the ocellus (*Oc*) is situated.

The labrum (fig. 35, *L*) has been carried on to the ventral surface of the body, and its anterior angle has become produced into a short stout, sharp spine, which is extremely small during the first *Protozoëa* stage.

As has been stated, the compound eye is represented at the second stage by a pigment-spot (fig. 35, *E*).

The mandibles (*M*), (figs. 27, 34, 35), have become reduced to cutting blades, which are visible in a dorsal view, and all traces of the *Nauplius* limb have disappeared.

During the first *Protozoëa* stage (Plate 3, figs. 28 and 29) it has only one denticle, which is large and pointed, and situated at the posterior angle of the cutting edge; but at the second *Protozoëa* stage (Plate 4, fig. 37) a number of smaller denticles have appeared in front of the long one. The mandibles are never quite symmetrical, but the outline of the left always differs a little from that of the right.

The external surface of the first maxilla of the first *Protozoëa* is shown in fig. 30, and the posterior surface of that of the second *Protozoëa* in Plate 4, fig. 38. It consists, at both stages, of a basal portion made up of two joints with cutting hairs (fig. 38, 1 and 2); a two-jointed endopodite (*en*), with three long slender hairs; and an exopodite or scaphognathite (figs. 30 and 38 *sc*), with three long slender hairs. In the first stage (fig. 30) the hairs of the scaphognathite are simple, but in the second stage (fig. 38) they are plumose.

The posterior surface of the second maxilla of the first *Protozoëa* is shown in Plate 3, fig. 31, and that of the second *Protozoëa* in Plate 4, fig. 39. It consists of a many-jointed basal portion (*b*), a two-jointed endopodite (*en*), and a scaphognathite or exopodite (*sc*). The whole inner edge of the appendage carries short stout hairs; the tip of the endopodite a few somewhat longer hairs; and the scaphognathite three slender plumose hairs, which are much longer in the second than in the first stage.

The first maxilliped (figs. 32 and 40) is very similar to the second antenna, and consists of a two-jointed basal portion, a four-jointed endopodite, and an unjointed exopodite. The inner edge is set with short stout hairs, which are simple in the first, but irregularly plumose in the second *Protozoëa* stage. The terminal joint of the endopodite carries four long slender simple hairs, and the tip of the exopodite four long straight slender hairs, which are plain in the first but regularly plumose in the second stage.

The second maxilliped of the first *Protozoëa* is shown in fig. 33, and that of the second *Protozoëa* in fig. 41. It is essentially like the first maxilliped in structure, but much smaller, and apparently of little functional importance.

In the second stage there is a small convoluted shell gland (fig. 35, *sg*), which appears to open at the base of the first maxilla; but the constant and violent movements of the limbs render it difficult to decide with confidence exactly what its relation to them is, and it is possible that its opening is upon the base of the second instead of first maxilla.

In the second *Protozoëa* stage the two pigment-spots (*p*) on the carapace become extremely dendritic, and a pair of anal pigment-spots (Plate 3, fig. 34, *pp*) make their appearance on the telson on each side of the anus.

At this stage the area, when the œsophageal muscles are attached to the carapace, is somewhat peculiarly marked by six little circles arranged in a pentagon, as shown, highly magnified, in fig. 35*a*.

The last Protozoëa *stage* (Erichthina).

The change from the last stage to the next one in the series was actually observed in several specimens, and more than fifty larvæ passed through it in the laboratory.

After the moult the larva, which is shown from the ventral surface in Plate 4, fig. 42, and in outline in fig. 42*a*, has the characteristics of DANA's genus *Erichthina.*

Its length, from the tip of the rostrum to the end of the telson, has increased to about $\frac{27}{1000}$ inch, and most of the increase is in the hind body. The carapace also is somewhat elongated (it was a little flattened by pressure in the specimen which was drawn), and the outline of the anterior edge is no longer regularly curved. At the base of the rostrum there is a slight eminence where the integument is pushed out a little by the optic ganglion, and at the outer angle there is a much larger eminence which is the rudimentary cornea of the compound eye. The eye itself is now represented by a large conspicuous pigment-spot (fig. 42*a*, *E*).

The appendages have undergone extremely little change, and they are, as before, as follows: the first antennæ (*A*), the second antennæ (*An*), the mandibles (*M*), the two pairs of maxillæ (*Mx.* 1 and *Mx.* 2), and two pairs of maxillipeds (*Mp.* 1 and *Mp.* 2). The second antennæ are still the chief organs of locomotion.

The hind body is much longer than it was at the stage before, and it is now somewhat longer than the carapace. It now consists of nine free segments and an unsegmented portion (*A* 5, 6). The first of the free segments (fig. 42, *Mp.* 3) is much narrower than any of the others, and its outer edges are marked by enlargements which appear to be the rudimentary appendages, the third pair of maxillipeds. None of the segments which follow it show a trace of the appendages, and the thoracic and abdominal ganglia are not yet visible.

The four segments which follow next after the one with the bud-like processes have rounded posterior edges, while the posterior edges of the next four are pointed. The later history seems to show clearly that those with rounded edges are the first, second, third, and fourth thoracic somites, and that the following ones are the first, second, third, and fourth abdominal somites. It will be seen, then, by a comparison of this with the earlier and later stages, that the somites of the body are all developed in regular order, from in front backwards, but that the first abdominal somite follows immediately after the fourth thoracic, while the fifth thoracic is never developed. At this stage the long unsegmented region (*A* 5, 6), represents the fifth and sixth abdominal segments and the telson. The two anal pigment-spots are larger than they were during the stage before, and from this time to maturity their colour is a dirty reddish-brown instead of black.

The "Zoëa" *stage* (Elaphocaris *stage of* Sergestes.)

After the next moult, which was observed in a great number of specimens, the larva passes into a stage which is directly comparable, so far as the appendages are concerned, with the *Elaphocaris* stage of *Sergestes*, although the most conspicuous features of the *Elaphocaris* larva, the long compound spines, are not present in *Lucifer*. It is now about $\frac{50}{1000}$ inch long, and it is shown in a dorsal view in Plate 5, fig. 44, and, more highly magnified, from below in Plate 4, fig. 43. In a side view (fig. 45) it still agrees pretty closely with fig. 35; its body is carried in the same attitude, and the antennæ are still the chief organs of locomotion. The fully-developed appendages are, as before, the first and second antennæ, the mandibles, two pairs of maxillæ, and the first and second pairs of maxillipeds, but the third pair of maxillipeds, four pairs of thoracic appendages, and the swimmerets or appendages of the sixth abdominal somite are now present as rudimentary buds.

The compound eye (figs. 43 and 45, *E*) is now well advanced in development, although there is as yet no trace of a stalk, and the cornea is simply a modified portion of the integument of the carapace.

The carapace is longer, narrower, and more rectangular in a dorsal view than it was at the last stage, and it makes only about one-third of the total length of the body of the larva. Its pigment-spots are very large, dendritic, and conspicuous, but their colour has changed from black to dark reddish-brown.

The anterior lobes of the stomach (fig. 44, *s*) have lengthened and approached each other on the median line, and they now reach forwards nearly to the optic ganglia.

The appendages which were present during the *Protozoëa* stage have essentially the same structure now, and the differences are very slight. The number of cutting hairs on the basal joints of the first maxilla (fig. 46) has increased; the hairs on its endopodite are plumose, and one of those carried by the scaphognathite is much longer than the other two. This is the case also with the second maxilla (fig. 47), and the hairs along its inner edge have become almost as long and slender as those at its tip. The first maxilliped (fig. 48) is almost exactly like that of the *Protozoëa;* but the second (fig. 49) is much more developed, and the hairs on its exopodite are plumose.

The hind body is now divided into its full number of segments; that of the third pair of maxillipeds (*Mp.* 3); the first, second, third, and fourth thoracic somites (*T* 1, *T* 2, *T* 3, and *T* 4); and the six abdominal somites, but the telson (*T*) is not yet completely distinct from the last abdominal somite. The thoracic somites are shortened and crowded together, and each of them carries a pair of bilobed buds, the rudimentary thoracic appendages. These buds are crowded together in a double row on the median line of the ventral surface of the body, and outside them is a pair of much larger buds (figs. 43 and 45, *Mp.* 3), bilobed also, but pointing backwards; the rudimentary third pair of maxillipeds.

The future history of the larva seems to show conclusively that the inner set of buds are, as indicated in fig. 43, the first four pairs of thoracic limbs or pereiopods. The side view (Plate 5, fig. 45) shows that there is no other pair in front of or behind them, and the fifth thoracic somite is entirely wanting, nor are its appendages present at any stage in the development of *Lucifer*.

The abdomen is much longer than it was at the last stage, and all its segments (fig. 43, *A* 1, *A* 6) are present, although the last one (*A* 6) and the telson (*T*) are not yet entirely separated.

The ventral surface of the sixth abdominal somite is armed with a pair of long stout spines over the base of the swimmeret, or sixth abdominal appendage, which is shown in fig. 43 as a long, bilobed pouch or bud, which reaches nearly to the tip of the telson. The third, fourth, and fifth abdominal somites carry, close to the anterior edge of the ventral surface, irregular groups of reddish-brown pigment-spots, which do not seem to be present in all specimens. The thoracic spots (fig. 44) and the anal spots (fig. 45) are usually a little more red than before, but they are nearly black in some specimens. The abdominal ganglia, which could not be distinctly made out in the last *Protozoëa*, are now very conspicuous, as shown in the ventral view (fig. 43). They lie near the posterior edges of the somites, and their halves are united in the median line, although the commissures between the ganglia are quite widely separated.

The spines on the telson have lengthened, but their number, arrangement, and relative size is the same as before. Their proximal ends from the base about half-way to the tip are marked by fine serrations, which appear to be short hairs, which have not been perfectly extended.

Schizopod or Sceletina *stage* (Acanthosoma *of* Sergestes).

Up to this time the mode of locomotion has been by means of short, jerking *Nauplius* leaps, and the two pairs of antennæ have been, as they were when the larva left the egg, the chief organs of locomotion. The structure of these appendages has remained extremely constant through all the moults, but they now change their character entirely, and lose their locomotor function.

The change which is undergone by the larva at the end of the *Zoëa* series is very much greater than it has been at any preceding moult, except that between the *Nauplius* and the first *Protozoëa*, and in some respects it is even greater than it was at that time. After the moult it is a Schizopod (Plate 6, fig. 50), about $\frac{70}{1000}$ inch long, with seven pairs of long jointed biramous swimming feet, fringed with long slender hairs. The swimmerets are also present as functional appendages, with long fringing hairs.

This stage differs from those which have gone before in this, that it persists with slight change for several moults, while there has been considerable change at each of the preceding moults. It is shown from below in fig. 50, as it appears immediately after the moult which follows the stage shown in fig. 43.

The figure was drawn from a *Zoëa* which was captured at the surface of the ocean, carefully examined and compared with fig. 43, and found to agree with it exactly. It was then placed alone in a small beaker of sea-water. The next day it was found to be moulting, and the drawing (fig. 50) was made from it immediately after the completion of the moult. Other specimens, like fig. 50, were kept until they changed their skins, and assumed a form a little larger than fig. 50, but similar to it in all respects except that the abdominal appendages were now present as small buds. Some of these were kept until they changed into larvæ like the one which is shown, less highly magnified, from the side, in fig. 54. The abdominal appendages were now quite long, but still rudimentary, and the general form of the larva from above or below, as well as the form, number, and arrangement of the thoracic appendages and mouth parts, was like fig. 50.

When seen from above or below (fig. 50) the carapace has nearly the same shape that it had during the *Zoëa* stages, but it now makes less than one-third of the total length of the body, and a side view (fig. 54) shows that it is now only a little deeper than the body, so that the basal joints of the thoracic limbs and maxillipeds are exposed below its inferior border. The posterior dorsal spine and the two postero-lateral spines have disappeared, and a pair of long antero-lateral spines (fig. 54, *s*), nearly half as long as the rostrum, have made their appearance underneath the eyes. The rostrum (fig. 50, *R*) has the same shape and about the same relative length as before, and the ocellus (*Oc*) is still present at its base.

The compound eye (*E*) is mounted upon a movable stalk, which is quite short during the first Schizopod stage, but it soon lengthens, as shown in fig. 55, which is a dorsal view of the anterior end of the carapace of the larvæ shown in fig. 54.

The first antenna has undergone more change at this than at all the previous moults together. It is now about as long as the carapace, and each of the two long cylindrical joints (fig. 50), which make up its basal portion, carries on its inner edge three long slender two-jointed delicately plumose hairs. The base of the proximal joint is swollen and carries a small hook-like process on its inner edge. The two long sensory hairs have disappeared from the tip, which is unsegmented, pointed, and ends in a bunch of short hairs. This appendage changes slightly with each moult, and in the third Schizopod stage (fig. 54) the distal half of the proximal joint (fig. 56) has separated from the proximal joint, so that the shaft is made up of three instead of two portions. The hook is still present on the swollen base of the first joint, and behind it the otocyst (*e*) has made its appearance. The terminal joint or flagellum has now lengthened, and it carries three long sensory hairs which spring from about the middle of its outer surface.

The changes which the second pair of antennæ undergo at this moult are even greater than those which take place in the first pair. Their locomotor function is lost; the long swimming hairs have disappeared; and in the first Schizopod stage (fig. 50) the appendage is quite rudimentary, unjointed, less than one-half as long as the first antenna, and divided into an exopodite and an endopodite which are nearly equal in length, although even at this stage the endopodite is a little the longest. Each ramus ends with a pair of very short hairs.

The appendage now changes with each moult, and in the third Schizopod stage (fig. 54) the exopodite has become a scale (fig. 57, *ex*) while the endopodite (*en*) has elongated, and now forms a seven-jointed flagellum, about as long as the first antennæ or the carapace. The basal joint (fig. 57, *b*) is thick and swollen, the two proximal joints of the flagellum (2 and 3) are short; the next (4) long, and the other four about equal in length, and about half as long as the joint (4).

Through all the Schizopod stages the structure of the labrum (*L*) is about as it was in the *Protozoëa* and *Zoëa*, and its interior angle is still produced into a short stout sharp spine.

The mandibles are cutting jaws with no trace of a palpus, and at the first Schizopod stage (fig. 51) the denticles are numerous and of nearly uniform size. In the last Schizopod stage (fig. 58) a second set of denticles has appeared on the outer surface of the blade a short distance from the cutting edge.

The first maxilla (fig. 52) is very much like that of the *Protozoëa* and *Zoëa*, but the cutting hairs upon the two basal joints (1 and 2) are more numerous, and a small slender plumose hair has appeared near the edge of each joint. The scaphognathite is small and has only two hairs, which are less regularly plumose than before.

The scaphognathite of the second maxilla (fig. 53, *sc*) is now rudimentary and has no hairs. The hairs on the inner edge of the appendage are shorter than they were during the *Zoëa* stage, and all of them are plumose and about equal in length.

The first maxilliped (fig. 50, *Mp.* 1) has not changed very much, although its joints are nearly absent. The exopodite is about as long as the endopodite, and all the hairs on the appendage are short and plumose.

The second and third maxillipeds and the four pairs of thoracic appendages are well developed, as a series of long biramous or Schizopod feet, which are essentially alike in form and structure, and, with the telson and swimmerets, now form the locomotor apparatus of the larva, which no longer swims by jerks but darts through the water with great rapidity, and is able to offer considerable resistance to the suction of a dipping tube. Each swimming foot consists of a two-jointed basal portion or protopodite, a long four-jointed endopodite, and a much shorter exopodite. The exopodite is flat, pointed, and its outer or distal half is marked by a series of six pairs of notches, or annulations, close together. The terminal joint carries a pair of long slender unplumose hairs, and a pair of similar hairs springs from each annulation, so that there are fourteen hairs in all on each exopodite, arranged so as to form a large fan-shaped paddle at the tip of the limb. The terminal joint of the endopodite is much shorter than the others, and it carries six long plumose hairs. The first appendage in this series, the second maxilliped (fig. 59, *Mp.* 2), is somewhat rudimentary: the endopodite is scarcely longer than the exopodite, and its hairs are short. The next or third maxilliped (*Mp*. 3) is more like those which follow, but its hairs are shorter. The first, second, and third pereiopods are about equal in length, and they have the typical structure which has just been described; but the endopodite of the fourth (*Pr*. 4), like that of the second maxilliped, is shorter than the exopodite, although its hairs are very long.

At the last Schizopod stage (fig. 54) the series of limbs, shown from above in fig. 59, is about as it is in the first stage, but the hairs on the endopodites of all the appendages, except the last, are short. A comparison of one of these appendages with the second antenna of the *Nauplius* or *Protozoëa* or *Zoëa* shows great similarity, and I am therefore disposed to believe that the long jointed ramus of the antenna is homologous with the long ramus of the thoracic limb, and consequently the endopodite.

The abdomen is very much longer in proportion to the carapace than it was at the "*Zoëa*" stage, and a comparison of figs. 50 and 54 with fig. 43 will show that it has become flattened from side to side, while its vertical thickness has greatly increased. All six somites are distinct, but at the first Schizopod stage there are no traces of any abdominal feet except the swimmerets, which are large and perfect. In the second Schizopod stage the first five pairs of pleopods are represented by short buds, and in the last Schizopod stage (fig. 54) they have nearly or quite their full size, but are still rudimentary.

The posterior edge of the ventral surface of each abdominal somite carries a couple of spines (fig. 50) pointing backwards. They are small on all the somites except the last, and they appear to correspond to those which, from their great size, have given the name *Acanthosoma* to the larva of *Sergestes* at the same stage of development. The sixth abdominal somite also has a small median dorsal spine.

The telson (*T*) is movable, greatly elongated, three times as long as wide, and its spines have become very small, although in number, arrangement, and relative size they agree with those of the *Zoëa* and *Protozoëa.*

The sixth pleopod or swimmeret consists of a short thick basal joint, a long flat exopodite which is serrated along its inner edge and free extremity, but smooth along its outer edge; and a flat endopodite serrated on both sides. Each serration carries a long slightly curved plumose hair, and the outer edge of the exopodite has a small tooth at its outer end. From the base to the tooth the outer border is nearly straight and parallel to the inner border, but the end of the appendage is prolonged into a rounded tip which reaches beyond the tooth. In the first Schizopod stage there are eight hairs on the inner border and four on the end of the exopodite, or twelve in all; and there are eight hairs on the endopodites, but the number of serrations and hairs increases rapidly with each moult, on each division of the limb, and they are much more numerous in the last Schizopod stage, as shown in fig. 54.

A large reddish-brown pigment-spot (fig. 54, *p*) has now appeared on each side of the fourth abdominal segment, and the anal spots are large, with a dull red tinge. The spots on the carapace disappear at the end of the *Zoëa* series.

The Mastigopus *stage.*

After the next moult the larva (Plate 7, fig. 60) assumes a form which is essentially like that of the adult, but with numerous slight differences, the most important of which are the shortness of the flagellum of the first antenna and the absence of the neck or elongation of the carapace. In these respects, as well as in the number, character, and relative size of the appendages, it now agrees very closely with the young *Sergestes* or *Mastigopus.*

The size of the thorax is reduced, while the abdomen has grown larger and longer. The exopodites of the maxillipeds and first three pairs of pereiopods have disappeared, together with every trace of the fourth pereiopod. The abdominal appendages are perfect; the first is made up of an elongated basal joint, which carries a single terminal branch of about the same length as the basal joint, but pointed and fringed with long slender swimming hairs. The four appendages which follow are each furnished with two terminal branches instead of one, but are similar in other respects. The larva now sheds its skin several times, and grows with each moult; but the process of change into the adult is, with the exception of the elongation to form the neck, simply a process of growth, as the appendages and somites all have essentially their adult character.

A larva about one-fifth of an inch long, two moults after the last Schizopod stage, is shown from the side, magnified about fifty diameters, in Plate 7, fig. 60. The first antenna (*A*) is a little more than twice as long as the eye-stalk, and consists of a stout three-jointed basal portion, which forms about two-thirds of the total length of the appendage, and a short, thin, two-jointed flagellum. The scale (*ex*) of the second antenna is only a little longer than the eye, while the flagellum (*en*) is more than half as long as the body of the animal, measured from rostrum to telson, and is made up of thirteen small joints and two thicker basal joints.

The carapace has elongated considerably, aud the neck (*n*) makes nearly half its length. The anterior end of the carapace has a dorsal rostrum (*R*), two much shorter lateral spines (*ls*), and a very small spine on each side close to the anterior edge and about half way between the rostrum and the lateral spine. The cephalic lobes of the stomach extend into the neck, and reach nearly to the basis of the eye-stalks. The coiled antennal gland (*g*) has made its appearance. The carapace proper (*c*) has a pair of anterior spines, but none on its posterior margin. The labrum (*L*) has a much greater relative size than it had during the Schizopod stages, but its spine disappears at the end of the last Schizopod stage. The mouth parts and thoracic limbs have their adult character, and will be noticed at length in the description of the adult. A reddish-brown pigment-spot has now appeared between the bases of the eye-stalks;

another at the base of the telson; and the dorsal surfaces of the fifth, fourth, and sometimes the third abdominal somites are irregularly marked, near their posterior edges, by patches of the same colour. The anal pigment-spots are of a dirty red colour.

The Lucifer *stage.*

The specimen from which fig. 61 was drawn was a little more than half an inch long, or about half as large as an adult specimen. It differs in several particulars, besides size, from an adult male, but in all respects except size and the presence of reproductive organs it is exactly like a mature female. Its appendages are like those which are shown in figs. 63 to 70, although these were drawn from an adult female specimen.

The adult structure of our American species has been described by FAXON ('Studies from the Biological Laboratory of the Johns Hopkins University,' vol. i., part iii.); but as he had only a single male specimen, which had been preserved in alcohol, his account was necessarily somewhat incomplete.

The first antenna (Plate 7, fig. 61, and Plate 8, fig. 66, *A*) is about as long as the carapace and neck, and it is divided into two nearly equal portions, the base (fig. 66, 1) and the flagellum (fig. 66, 2). The base is divided into three joints, the first about as long or a little longer than the eye, the second much shorter, and the third still shorter. The large ear occupies the centre of the proximal end of the first joint. On the outer end of the first joint and on the second there is a row of six short, equal, plumose hairs, three on each joint. The flagellum is made up of ten joints; the first and second are thicker than the others, and the first carries two and the second three sensory hairs. The terminal joint of the flagellum is much longer than the other, and carries a few very short hairs at its tip.

The second antenna (figs. 61 and 66, *An*) is, in the fully-grown specimen, almost twice as long as the first, and nearly or quite as long as the body. It consists of a very short basal joint (fig. 66, 3), which carries the scale (*ex*) and the flagellum (*en*). The scale is somewhat longer than the eye, flat and narrow, and its inner edge carries nine and its tip three long, slender, plumose hairs, which are about half as long as the scale itself. The flagellum tapers gradually from the base to the tip, and is made up of twenty-four joints, each of which carries a pair of very short hairs. The joints at the tip of the flagellum are a little longer than those at the base. The living animal usually carries these appendages extended before it, and diverging a little at their tips. It occasionally throws them back along the sides of the body, but only for an instant at a time.

The eye-stalk tapers gradually from the base to the tip, and there is no abrupt distinction between the stalk and the eye proper, as there appears to be in other species. The length of the eye, with its stalk, is a little less than that of the true carapace.

The neck makes a little more than three-fifths of the total length of the carapace, and its vertical diameter is more than half that of the thorax. It has a median dorsal rostrum (fig. 61, *R*), which is much smaller relatively in the adult than in the young, and two antero-lateral spines (*ls*). About half-way between the rostrum and the lateral spine the anterior edge of the neck has an extremely minute spine on each side, as in the younger stage last described. The cerebral ganglia (*cg*) occupy the ventral half of the anterior end of the neck, and the long commissures can be seen at *co.* running back to join the ventral nervous system. The cephalic lobes of the stomach (*s*) and the antennary gland (*g*) occupy the dorsal portion of the neck.

The true carapace (*c*) does not reach down on to the sides of the body as far as the basal joints of the thoracic limbs and mouth parts, and both these and their ganglia (figs. 75 and 76, *tg*) are visible below its free edge. Its edges are smooth, but there is a small spine at its anterior end.

The labrum (fig. 61, *L*) is massive and prominent, but there is no trace of a spine.

The inner surface of the mandible (fig. 62) is marked by a number of parallel ridges, one for each denticle; and there is a second, and a faint trace of a third, series of denticles on the outer surface (fig. 63). There is no trace of a mandibular palpus.

The scaphognathite of the first maxilla (fig. 64) has disappeared, the endopodite is rudimentary, while the second basal joint is very much larger than the first, and carries about fifteen stout short hairs arranged in three rows. The first joint has four much larger unequal hairs, which are serrated. The outer edge of the first and both edges of the second joint carry a single delicate plumose hair each. Fig. 65 shows the inner surface of the second basal joint.

The second maxilla (Plate 8, fig. 67) is more like that of the larva. There is a three-jointed inner portion with short stiff hairs, and an extremely large scaphognathite (fig. 67, *sc*), which is long and narrow, and united to the body of the appendage by a very narrow stalk. The outer end carries three rather stiff, short, plumose hairs, and five similar but somewhat longer hairs arise from the inner surface between the outer end and the area of attachment. The inner end carries four plumose hairs, three of which are almost as long as the scaphognathite itself, while the fourth appeared to be broken off in the four specimens which I dissected out.

The first maxilliped is a short, stout, two-jointed appendage (Plate 8, fig. 68), convex on its outer but flat on its inner surface, and fringed with short, stout, plumose hairs.

The second maxilliped (Plate 7, fig. 61, *Mp.* 2, and Plate 8, fig. 70) is a long jointed limb, bent into a knee, and formed of six joints. It is fringed by long plumose hairs, which, on all the joints except the first and second, are arranged in a single row. The first and shortest joint has no hairs; the next, or second, has one row of five and one row of three; the next, or third, has six hairs; the next, or fourth, and the fifth have ten each; and the terminal joint has six.

The next or third maxilliped (fig. 61, *Mp.* 3) is a long, slender, six-jointed limb, with a double row of short hairs.

The first pereiopod (fig. 61, *Pr.* 1) is four-jointed, and shorter than the last maxilliped.

The second and third pereiopods (*Pr.* 2 and *Pr.* 3) are nearly equal, and twice as long as the first; they are four-jointed, have a double row of small hairs along the anterior edge, and the last ends in a small curved hairy claw.

They exhibit no trace of gills or of endopodites, and there is no stump to indicate the position of the fourth pereiopod, which disappeared at the end of the Schizopod period.

The first abdominal appendage of immature specimens or of mature females (Plate 9, fig. 74, *Pl.* 1) is made up of a thick basal portion, which is unjointed in young specimens but two-jointed in mature ones, and a pointed annulated terminal portion which is fringed with swimming hairs. In the nearly grown but immature male (fig. 76) there is a little bud or projection (*a*) near the base of the anterior surface of the long basal joint. In the sexually mature male (fig. 75) this bud has become the clasping organ which has been described by MILNE-EDWARDS, DANA, SEMPER, DOHRN, CLAUS, FAXON, and others; and another smaller process or tooth has appeared upon the distal one of the two joints into which the base of the limb has now divided.

The second, third, fourth, and fifth pleopods consist, in the young of both sexes, and in the mature females, of a long unjointed basal portion and two hairy terminal branches. In the adult male the second pleopod has a third and smaller terminal branch, as CLAUS has pointed out (Zeit. f. Wiss. Zool., xiii., 434).

The first, second, third, fourth, and fifth abdominal somites end below in short spines, and they are all about equal in length, except the fifth which is nearly twice as long as any of the others. It has a median dorsal spine on its posterior edge, and the very young specimens also have a pair of postero-lateral spines, as shown in Plate 7, fig. 60. In older specimens this pair of spines disappears, as shown in Plate 9, fig. 72, and in the adult female the somite undergoes no further change. When the male reaches sexual maturity, however, the lower edge of the somite becomes produced, as described by DANA, on each side into the hooks shown in fig. 73. In our species the smaller one of these hooks is near the middle of the somite, and the larger one about half way between it and the posterior edge.

As shown in figures 72 and 73, the telson of an adult specimen is only about half as long as the swimmerets. The tip of the telson of an adult female is shown from above in fig. 71.

In the male the telson becomes somewhat bent (fig. 73, *T*) as maturity is reached, and a rounded anal papilla becomes developed in its lower surface, while the telson of the adult female remains like that of immature specimens of both sexes.

The exopodite of mature specimens usually has about twenty hairs, and the endopodite sixteen. The exopodite is longer and wider than the endopodite, and it is alike in both sexes until maturity is reached, when it becomes somewhat modified in the male, This sexual difference has been pointed out by DOHRN (Zeit. Zool., xxi., 1871, p. 358), but it seems to have escaped the notice of all other observers.

In the young and in the mature female (fig. 72) the rounded tip projects beyond the tooth (*a*), but as the male approaches maturity the outer edge lengthens, thus pushing the tooth out, as shown in fig. 73, until the end of the appendage becomes square instead of rounded. It is extremely interesting to notice that in *Lucifer*, as in so many other animals, the adult female is infantile in all the secondary points of difference from the male.

General view of the metamorphosis of Lucifer.

A review of the facts which have been described in this section indicates that some of the changes are much more significant than others, since the number of moults is much greater than the number of distinct larval type.

The meta-*Nauplius* is obviously a *Nauplius* with the rudiments of structures which are to appear after the moult, and it must therefore be regarded as a *Nauplius* prepared for the change into a *Protozoëa*, rather than a distinct stage of development.

There is no such break between the first *Protozoëa* and the last *Zoëa* as there is between the first *Protozoëa* and the *Nauplius*. The rudimentary pereiopods and swimmerets of the so-called *Zoëa* are nothing but a preparation for the next stage of development, and the supposed necessity for finding a stage which can be directly compared with the *Zoëa* of the higher Decapods does not justify us in making two larval types out of the unbroken series of *Protozoëa* and *Zoëa* forms.

It is obvious that the three Schizopod stages are modifications of a single larval type, and the presence of rudimentary pleopods in the second and third stages must be regarded as a preparation for the next stage of development.

There is no abrupt break between the so-called *Mastigopus* and the young *Lucifer* when it is a little older and the neck has appeared.

On the other hand, there is a real break between the *Nauplius* and the *Protozoëa*, and the change from one to the other is accompanied by profound structural changes. This is the case also with the transition from the *Zoëa* to the Schizopod stage; and with that from the Schizopod stage to the young *Lucifer* stage. The same thing is true to a lesser degree of the change from the immature *Lucifer* to the adult male.

The metamorphosis may then be divided into the following well-marked stages, each of which except the last, and in all probability the last also, persists through more than one moult:—

1. A *Nauplius* stage.
2. A *Protozoëa* stage.
3. A Schizopod stage.
4. An immature *Lucifer* stage, which persists in the female.
5. An adult male stage.

If we neglect the features which, at the end of each stage, make their appearance as preparation for the next, we may describe each stage as follows :—

The *Nauplius* has three pairs of locomotor appendages, the first antennæ, the second antennæ, and the mandibles; and there is a large labrum without a spine, and the carapace and telson are absent. There is an ocellus, but no compound eyes.

The *Protozoëa* has two pairs of antennæ, which are like those of the *Nauplius.* The mandible is reduced to a cutting blade. There are two pairs of biramous maxillæ, with scaphognathites, and two pairs of biramous maxillipeds. There is a long hind body, ending in a flat telson. The labrum has a spine. The carapace is large, and has a rostrum, a median dorsal and two lateral posterior spines; and its free edges reach down beyond the basal joints of the appendages. There is an ocellus, but no stalked eyes.

The Schizopod stage is characterized by the great change in the two pairs of antennæ, which are no longer like those of the *Nauplius,* but have the characteristics of those of the adult. All the mouth parts and four pairs of thoracic limbs are present, and all posterior to the first pair of maxillipeds are biramous and locomotor. The abdomen has six somites and a movable telson. The swimmerets are present, but the other abdominal appendages are not.

The ocellus persists, but the stalked eyes are also present. The carapace has a rostrum and two antero-lateral spines, but those at the posterior edge have disappeared. The edges of the carapace do not reach over the basal joints of the thoracic limbs, and the body is flattened vertically. The labrum still has a spine.

The young *Lucifer* and the adult female have a long flagellum on the first antenna, a flagellum and scale on the second; the ear and antennary gland are present; the neck is elongated. The fourth pereiopod has disappeared, and the others, as well as the maxillipeds, have lost their exopodites. The first pleopod has one terminal branch, the next four two branches each; the sixth abdominal somite has a smooth lower edge. The telson is straight and the outer end of the exopodite of the swimmeret is rounded.

The adult male has a clasping organ on the first pereiopod, three rami on the second, two teeth on the lower edge of the sixth abdominal somite, a square end to the exopodite of the swimmeret, and a bent telson.

It is true that these five stages merge into each other somewhat, and that they are complicated by the presence of the rudiments of organs which are be functional at the next stage; but after all these secondary modifications are allowed for, it will be seen that each stage is sharply and definitely marked, and separated by a pronounced gap from the stages before and after.

The significance of these five stages can be best inquired into after the corresponding stages of other *Sergestidæ* have been examined, and I will return to the subject further on, in a section on the general relationships of the group.

IV. History of the Appendages of Lucifer.

For convenience of reference I will now describe the changes which each appendage undergoes at each stage of development, going over the same ground once more, but in a different way.

The first antenna.

In the egg *Nauplius* (figs. 21, 23, and 24, *A*) this appendage is unjointed, more than half as long as the body, and it carries a terminal tuft of hairs.

In the first free *Nauplius* (fig. 25, *A*) it consists of five nearly equal joints; it is nearly as long as the body of the second antenna, and its tip carries two long simple hairs and two much smaller hairs.

In the last *Nauplius* stage, or meta-*Nauplius* (fig. 26) the joints have disappeared; it is only about two-thirds as long as the body, and it carries only the two long hairs at the tip.

In the first *Protozoëa* stage (fig. 27, *A*) it is made up of a long cylindrical basal joint with a few short hairs, and a much shorter terminal joint, which is pointed, and carries the two long hairs as before.

The structure of the appendage does not change until the end of the *Zoëa* series, and it is shown at *A* in figs. 34, 42, 43, and 44.

At the first Schizopod stage (fig. 50, *A*) the basal portion is made up of one very long cylindrical joint, with a hook near its swollen base, and a much shorter distal joint. Three long, two-jointed, plumose hairs spring from the inner edge of the second joint, and three more from the inner edge of the distal third of the basal joint. The terminal portion has lost the two long hairs which it had at earlier stages.

In the last Schizopod stage (fig. 54, *A*, and fig. 56) the distal third of the basal joint has separated off as a distinct joint (fig. 56, 2) upon which the three hairs are situated. The ear has made its appearance, behind the hook, on the swollen base of the first joint. The terminal joint (4) carries three sensory hairs, which arise upon its outer surface about half way between its tip and base.

In the *Mastigopus* stage (fig. 60) the terminal joint has lengthened to form a two-jointed flagellum, and the appendage is more than twice as long as the eye.

In the young specimens which have attained to the adult form (fig. 61, *A*) the appendage is about as long as the carapace and neck, and in the adult (fig. 66, *A*) the flagellum (2) is about as long as the basal portion (1). It consists of ten joints, the terminal one longest, and the first and second thick. The first carries two and the second three sensory hairs.

The basal portion is thick, cylindrical, three-jointed, with six plumose hairs, and the ear nearly fills the enlarged base.

The second antenna.

In the egg *Nauplius* (fig. 24, *An*) this is unjointed, more than half as long as the body, divided into two nearly equal rami, with hairs at their tips.

In the first free *Nauplius* stage (fig. 25, *An*) a two-jointed basal portion carries a three-jointed exopodite and an eight-jointed endopodite. The appendage is nearly as long as the body, the two rami are about equal in length, and each has three long simple hairs at its tip. In the last *Nauplius* stage (fig. 26) the joints are obscure; the endopodite is longer than the exopodite; it has long hairs along its side, and those at the tip are plumose. In the *Protozoëa* stages (figs. 27, 34, 35, 42, 43, and 45, *An*, and fig. 36) it consists of a two-jointed basal portion (fig. 36), which carries an unjointed exopodite (*ex*) with long, slender, non-plumose terminal hairs, and an eight-jointed endopodite (*en*) with eight long hairs arranged along its side and tip. The first and second joints are very short, while the other six are longer and nearly equal.

In the first Schizopod stage (fig. 50, *An*) the appendage is rudimentary, its joints are absent, and the exopodite is almost but not quite as long as the endopodite. The appendage is only half as long as the first antenna. In the last Schizopod stage (fig. 54, *An*, and fig. 57) the exopodite has become a scale, which is only half as long as the seven-jointed flagellum which has become developed from the endopodite; the basal joint is simple, very large, and the appendage is as long as the first antenna.

The flagellum now grows rapidly, and in the adult (fig. 61, *An*, and fig. 66, *An*) it has twenty-four joints, and is more than half as long as the body. The antennal gland opens into its base, and the scale is longer than the eye, and carries twelve long plumose hairs.

The mandible.

In the egg *Nauplius* (fig. 24, *M*) this is biramous, unjointed, and tipped with hairs. In the first free *Nauplius* (fig. 25, *M*) it is short, and made up of a stout basal joint; a two-jointed exopodite with three long slender hairs, two of which are carried by the terminal and one by the proximal joint; and a shorter endopodite with three long simple hairs. In the last *Nauplius* stage (fig. 26, *M*) the joints of the exopodite have disappeared, the three hairs on the endopodite have lengthened and become plumose, and the inner edge of the basal joint carries a hook or blade. From the beginning of the *Protozoëa* series to maturity the mandible is a cutting blade, with no trace of a palpus, and the number of its denticles gradually increases with age.

The metastoma.

The manner in which the metastoma originates in the *Nauplius* as a pair of buds similar to those which become the maxillæ, as well as the fact that it persists in closely-allied forms as a pair of limb-like structures, seems to show, as CLAUS has pointed out ('Untersuchungen,' &c., p. 15), that the Decapod metastoma is morphologically a pair of appendages; that it has been formed by the simplification and union of structures homologous with the limbs; and that this pair of appendages was originally furnished with a body-somite and a pair of ganglia. CLAUS's reason for the homology is the resemblance between the Decapod *Protozoëa* and the larva of Phyllopods and Copepods, and the manner in which these parts are developed in the Nauplii of *Lucifer* and *Euphausia* seems to be an additional reason for accepting his view.

The first maxilla.

This appendage is rudimentary during the *Nauplius* stages, but, as shown in fig. 21, *Mx.* 1, it is represented by a pair of buds several hours before birth.

In the *Protozoëa* and *Zoëa* series it has the form shown in fig. 46, which was drawn from the appendage of a larva in the last *Zoëa* stage. Its characteristics are developed gradually, and it is somewhat simpler during the earlier *Protozoëa* stages than it is in fig. 46. Fig. 30 shows it as it appears in the first *Protozoëa* when seen from the outside. It consists of a basal portion (fig. 46) made up of two joints (1 and 2), which carries a short obscurely-jointed endopodite (*en*) and a scaphognathite (*sc*). In my description of this and the other mouth parts of *Lucifer* I have accepted CLAUS's homology ('Untersuchungen,' &c., p. 16), and regard the two basal joints as the equivalent of the basal portion of the antenna, or of one of the thoracic limbs; the jointed palpus as the homologue of the inner ramus of the antenna, or the limb proper of one of the thoracic appendages; and the scaphognathite as the homologue of the exopodite of one of the thoracic appendages, or of the antennæ. In all these appendages the exopodite is shorter than the endopodite, unjointed, and set with long hairs, the plumose character of which is well marked. The scaphognathite of the maxilla agrees with the exopodite of the second antenna and of the other appendages in this respect, while the palpus of the maxilla agrees with the endopodite of the second antenna, and with that of the mandible of the *Nauplius* and with the thoracic limbs of the adult *Lucifer*, in consisting of several joints with one or more, usually simple, hairs at each joint.

The inner edges of the basal joints of the maxilla carry cutting hairs, and the second joint is largest. The endopodite carries five long slender hairs which are simple in the earlier and plumose in the later stages. The scaphognathite carries three hairs which are equal and simple in the earlier *Protozoëa*, but plumose in the *Zoëa*, where one is very much longer than the other two.

The structure of this appendage undergoes extremely little change from the time it appears in the *Protozoëa* to maturity. In the Schizopod larva (fig. 52) the second basal joint (2) has become much larger than the first (1), and its cutting hairs are more numerous than before; a small slender plumose hair has made its appearance on the edge of each joint. The endopodite (*en*) is obscurely three-jointed, and the scaphognathite (*sc*) has only two long plumose hairs.

In the adult (fig. 64) the scaphognathite is absent; the endopodite is rudimentary and the second joint of the base (2) is very much wider than the first (1), and has fifteen cutting hairs arranged in three rows, while the first joint has only four very much longer serrated cutting hairs. The basal joint has only one plumose hair as before, but the second joint has one on each side of the blade.

The second maxilla.

The second maxilla is present as a bud (fig. 24, *Mx.* 2) in the egg, and it becomes functional in the first *Protozoëa*, and persists without very much change to maturity.

In the first *Protozoëa* (fig. 31) it has a long, many-jointed basal portion (*h*), with slender simple hairs on its inner edge; a two-jointed endopodite (*en*) with three simple hairs on its tip, and two on the second joint; and a small scaphognathite with plumose hairs.

In the last *Zoëa* (fig. 47) the hairs on the inner edge are plumose, and one of the three hairs on the small scaphognathite is much longer than the others.

In the Schizopod stage (fig. 53) the limb is thick and long, the scaphognathite is rudimentary, and the endopodite is small, and has no terminal hairs.

In the adult (fig. 67) the endopodite and all but three of the joints of the basal portion are absent. The first of these (3) is the largest and has a broad edge, with a number of cutting hairs, while the others (2 and 1) are narrow and have three hairs each. All these hairs are simple. The scaphognathite is elongated, and is now about as long as the body of the appendage, to which it is joined by a narrow neck. The inner end has four plumose hairs, three of which are about as long as the appendage, while the fourth was short and apparently broken in all the specimens which I examined. The outer half of the scaphognathite has three short straight plumose hairs on its outer end, and five somewhat longer ones on its inner side.

The first maxilliped.

The first maxilliped is represented by a bud in the egg *Nauplius* (fig. 21 *Mp.* 1) and it becomes functional in the first *Protozoëa*, and then consists (fig. 32) of an unjointed exopodite (*ex*) with four long terminal hairs; a four-jointed endopodite (*en*), with three long terminal simple hairs, and a shorter hair springing from the inner edge of each joint; and an obscurely two-jointed basal portion with short simple hairs on its inner edge.

In the *Zoëa* stage (fig. 48) the hairs on the inner edge and on the exopodite are plumose, and the endopodite is long and six-jointed.

In the Schizopod stage (fig. 50, *Mp.* 1) the joints are obscure; the exopodite is nearly as long as the endopodite; all the hairs are plumose, and about equal in length, and there is a double row along the inner edge of the appendage.

In the adult the appendage (fig. 69) is extremely simple, short, stout, two-jointed, flattened on its inner and rounded on its outer surface, with a fringe of short, stout, equal, plumose hairs around the edge of the flattened surface.

The second maxilliped.

It is difficult to decide with certainty whether this appendage is represented by a bud in the *Nauplius* or not. If the first pair of buds become the metastoma, as seems probable from their position with reference to the mandibles and from the analogy of the *Euphausia nauplius*, the second pair of maxillipeds are not represented, but if the first pair of buds are the rudimentary first maxillæ the last pair are the second maxillipeds. At any rate the appendages are present in the first *Protozoëa* (fig. 33), and they are essentially like the first pair, but much smaller.

In the last *Zoëa* stage (fig. 49) they are larger, although still smaller than the first, and their inner edges carry only three short hairs which are not plumose.

In the Schizopod stage (fig. 59, *Mp.* 2) a long basal joint carries a four-jointed endopodite and an unjointed exopodite of nearly equal length. The outer half of the exopodite is fringed by fourteen long, simple hairs, and the terminal joint of the endopodite has a few short plumose hairs.

In the next stage the exopodite is absent, and the long six-jointed limb (fig. 70) is bent into the shape which is so characteristic of the adult *Sergestidæ*.

The basal joint (1) is quite short and stout. The next joint (2) is longer and has five plumose hairs, almost as long as the joint, on one side and three on the other.

The next joint (3) is the longest, and carries six plumose hairs. The next (4) is about as long as the second, and the bend in the limb occurs in this joint and between it and the third. It carries ten plumose hairs about as long as those in the other joints, and arranged in a single close rank. The fifth and sixth joints are shorter than any of the others except the first; they are about equal in length, and the fifth carries ten, the sixth six long plumose hairs.

The third maxilliped.

This appendage makes its appearance as a bilobed rudiment (figs. 43 and 45, *Mp.* 3), at the end of the *Zoëa* series, and it becomes developed into a Schizopod foot, at the next or first Schizopod stage (fig. 59, *Mp.* 3). A stout basal portion which appears to be two-jointed, carries an unjointed exopodite, and a four-jointed endopodite. The latter branch is the longest, and its tip carries four rather short plumose hairs. The outer half of the exopodite carries fourteen long simple hairs.

At the end of the Schizopod period the limb loses its exopodite entirely, lengthens and becomes a slender six-jointed leg, fringed by a double row of short hairs, as shown in fig. 61, *Mp.* 3.

The history of this appendage in *Lucifer* shows that that there is no reason, except the arbitrary system borrowed from the higher Decapods, for classing this appendage with the mouth parts, instead of with the thoracic limbs.

It appears much later than the first and second pairs of maxillipeds, or at the same time with the thoracic limbs. It agrees with these latter in all its subsequent changes and in its adult structure, and must be regarded as forming one of the thoracic series. I have employed the recognised name, third maxilliped, to prevent confusion, but the appendage is in no sense a mouth part. In fact, the only reason for holding that the missing appendage in *Lucifer* is the fifth pereiopod, instead of the last maxilliped, is the tacit assumption that the appendages must follow a definite serial order from in front backwards. We do not know that this assumption is justifiable in all cases, and it is therefore perfectly possible that the appendage which is usually called the third maxilliped of *Lucifer* may really be the first pereioped. I think the probability is in favour of the accepted homology, but the use of the term "third maxilliped" in the present paper for the appendage in question must not be regarded as evidence that the homology is accepted without question.

The pereiopods.

At the end of the *Zoëa* series four pairs of pereiopods, the first, second, third, and fourth, are represented by buds (figs. 43 and 45), while the fifth is entirely absent, as DANA pointed out in the 'Report on the Crustacea collected by the United States Exploring Expedition,' p. 634. WILLEMÖES-SUHM (Proc. Roy. Soc., vol. 24, p. 134), calls attention to the same fact: the total absence of this somite at all stages of development. In the Schizopod stage each of these appendages is biramous (fig. 59), and similar to the last maxilliped, although the first three pairs (fig. 59, *Pr.* 1, *Pr.* 2, and *Pr.* 3) are longer.

At the end of the Schizopod series of stages the entire fourth pair and the exopodites of the other three pairs disappear, and the endopodites lengthen to form the long slender limbs of the adult (fig. 61, *Pr.* 1, *Pr.* 2, and *Pr.* 3). They are four-jointed, with a double row of short hairs along the anterior edge, and the first is only half as long as the second and third, which are nearly equal, and almost as long as the carapace and neck. The third ends in a short, curved hairy claw, too small to be shown in the figure.

The first abdominal appendage.

This is present as a rudimentary bud at the end of the Schizopod series, but does not become functional until the *Lucifer* form is reached. In the young it consists of a long unjointed base, and a single pointed tip, fringed with swimming hairs (fig. 61, *Pl.* 1). In older specimens the basal portion divides into two joints, and in the young male or the young or mature female the appendage has the form shown in fig. 74. As the male approaches maturity a small process, shown in fig. 76, appears on its anterior face, and becomes modified in the mature male into the clasping organ (fig. 75, *c*), while a second process (*d*) appears a little nearer the tip of the limb.

The second abdominal appendage.

This appears at the same time with the first, and developes two terminal branches. In the mature male a third shorter one is added.

The third, fourth, and fifth abdominal appendages.

These all develop at the same time with the first and second; they have two terminal branches and are alike in both sexes.

The sixth abdominal appendage.

This is present as a rudiment in the last "*Zoëa*," and it becomes fully developed in the first Schizopod larva.

It consists of a basal joint which carries a long, wide, and flat exopodite, and a narrower shorter endopodite.

In the young and in the mature female the outer end of the exopodite is rounded, but it is nearly square in the mature male.

The labrum.

The labrum is large and conspicuous in the *Nauplius*, but it has no spine. The spine is present from the first *Protozoëa* stage to the last Schizopod stage, but it is absent in the adult.

The compound eyes.

These make their appearance as rudiments in the last *Protozoëa*, but they are not perfectly developed or stalked until the last Schizopod stage. The homology of the stalked eyes of the Malacostracan has been a matter of some uncertainty. They are usually enumerated in the list of appendages, and the typical Crustacean is supposed to have a corresponding somite. CLAUS has pointed out ("Zur. Kenntniss der Malacostrakenlarva," Würzb. Zeitschr. ii., 1861. p. 33) that no especial taxonomic importance can be attached to their presence or absence; and their mode of origin in *Lucifer* certainly gives no support to the view that they have been produced, like the mandibles, by the gradual specialisation of a pair of ordinary appendages. They do not resemble ordinary appendages at any stage, but are formed directly, and the fact that the period of their development is spread over several moults renders their history quite different from that of the appendages. As I shall show further on, serially homologous organs do not necessarily owe their resemblances to inheritance from the unspecialised organs of a remote ancestor, and I think that the presence of a distinct occular segment in *Squilla* compels us to recognise an homology between the stalked eye and an ordinary appendage, although it is no doubt true that all the groups in which stalked eyes occur cannot be traced back to a common stalked-eyed ancestor, and also true that the stalked eyes themselves cannot be traced back to ordinary appendages.

The ocellus.

This is present from the first *Nauplius* stage to the end of the Schizopod series.

Explanation of Table I.

This table is designed to show at a single view the condition of each appendage at each stage of development.

For convenience I have included the compound eyes, the ocellus, and the labrum, but do not wish to imply that these structures are or are not homologous with ordinary appendages, and I have omitted the metastoma, although I have no doubt that this should be included in a list of the appendages.

In the table the word "same" indicates that the condition of the appendage is the same as it was at an earlier stage, and does not refer to other appendages in the same vertical line.

TABLE I.—Comparative table to show the history of the appendages of *Lucifer*.

	Nauplius.	First *Protozoëa* or *Erichthina* Stage.	Last Zoëa.	First Schizopod or *Sceletina* Stage.	Last Schizopod.	Young of both sexes and adult female.	Adult male.
Compound eye	Absent	Same	Not stalked	Stalked	Same	Same	Same
Ocellus	Present	Same	Same	Same	Same	Absent	Absent
First antenna	Has nauplius form	Same	Same	Adult form, but rudimentary	Adult form	Same	Same
Second antenna	Has nauplius form	Same	Same	Rudimentary	Adult form	Same	Same
Labrum	No spine	Spine	Same	Same	Same	No spine	Same
Mandible	Biramous limb	Adult form	Same	Same	Same	Same	Same
First maxilla	Bud	Biramous	Same	Same	Same	Scaphognathite absent, endopodite rudimentary	Same
Second maxilla	Bud	Biramous	Same	Endopodite and scaphognathite rudimentary	Same	Endopodite absent, scaphognathite large	Same
First maxilliped	Bud	Biramous	Same	Same	Same	Rami absent	Same
Second maxilliped	Bud	Biramous	Same	Same	Same	Exopodite absent	Same
Third maxilliped	Absent	Same	Bud	Biramous	Same	Exopodite absent	Same
First pereiopod	Absent	Same	Bud	Biramous	Same	Exopodite absent	Same
Second pereiopod	Absent	Same	Bud	Biramous	Same	Exopodite absent	Same
Third pereiopod	Absent	Same	Bud	Biramous	Same	Exopodite absent	Same
Fourth pereiopod	Absent	Same	Bud	Biramous	Same	Absent	Same
Fifth pereiopod	Absent	Same	Same	Same	Same	Same	Same
First pleopod	Absent	Same	Same	Same	Bud	One ramus	Clasper
Second pleopod	Absent	Same	Same	Same	Bud	Two rami	Three rami
Third pleopod	Absent	Same	Same	Same	Bud	Two rami	Same
Fourth pleopod	Absent	Same	Same	Same	Bud	Two rami	Same
Fifth pleopod	Absent	Same	Same	Same	Bud	Two rami	Same
Sixth pleopod	Absent	Same	Bud	Biramous	Same	Exopodite round at tip	Exopodite square at tip

V. The Metamorphosis of Acetes.

While I was studying the development of *Lucifer*, I found during the month of September a few specimens of the very similar larva which is shown from above in Plate 9, fig. 79, and from the side in fig. 78.

Several specimens were placed by themselves in tumblers of sea-water, where they passed through the stages shown in Plate 11, figs. 84, 85, and 90. Only one of my specimens reached this last stage, and as this one moulted on the last day of the season I was not able to trace it any further, and as I collected no adult specimens of the same kind, its precise systematic position must at present remain in some uncertainty. The close similarity which I shall point out between its larval stages and those of *Lucifer* and *Sergestes* renders it very probable that it is a Sergestid, and the analogy of these forms also indicates that the larva shown in fig. 90 has in all probability nearly or quite attained to the mature form. This larva differs from the other two forms in the possession of small claws at the tips of the last three pairs of pereiopods, and as this is characteristic of Milne-Edwards' genus *Acetes*, and only three genera of Sergestidæ—*Lucifer*, *Acetes*, and *Sergestes*—have been described, I think we may conclude that we have to do with the development of an American species of this genus. At any rate, whatever the systematic position of the adult may be, the fact that the *Protozoëa* is in most respects intermediate between the simple *Protozoëa* of *Lucifer* and the extremely modified *Protozoëa* of *Sergestes*, gives this form so much interest that it seemed best, for the sake of comparison, to embody all that I was able to learn about its metamorphosis in the present paper.

At the earliest stage which was observed, the larva (figs. 77, 78, and 79) is a "*Zoëa*" $\frac{67}{1000}$ inch long, and a comparison of fig. 79 with fig. 44, or of fig. 77 with fig. 43, will show that it is essentially like the last *Zoëa* of *Lucifer*, although the minor differences are both numerous and conspicuous.

The number of somites and appendages is alike in both forms, and the appendages are alike in most respects, although each one of them shows distinctive characteristics of its own.

The carapace (fig. 79) makes about one-half the length of the body, and it is much more flattened than it is in *Lucifer*. It has a rostrum (*R*) and a median dorsal spine, but the postero-lateral spines (*sp.*) point outwards and backwards, instead of directly backwards, and there are a pair of anterior spines as long as the rostrum, projecting over the eyes. The two large pigment-spots which give such a characteristic appearance to the carapace of *Lucifer* are entirely absent, and the thoracic segments and appendages are covered by its posterior edge.

The eyes are mounted upon distinct stalks, while they are sessile in *Lucifer* at the same stage.

The abdominal somites are short and wide, and coloured by bright-red pigment-spots, and their lower edges are produced into strong projecting spines.

The telson is deeply cleft, and its halves diverge from each other like a swallow's tail feathers, so that the posterior ends of the rudimentary swimmerets are visible between them in a dorsal view, as shown in figs. 77, 78, and 79. The spines on the telson are similar in arrangement to those of *Lucifer*, but much longer.

A comparison of the Lucifer *Zoëa* (fig. 44), the Acetes *Zoëa* (fig. 79), and the Sergestes *Zoëa* (CLAUS's 'Crustacean-System,' taf. vi., fig. 1) at the same stage of development, brings out the extremely interesting fact that the *Acetes* larva stands between the very simple *Zoëa* of *Lucifer* and the remarkably complicated *Elaphocaris* larva of *Sergestes* in nearly every feature in which the two differ. In *Lucifer* the eyes are sessile; in *Acetes* they have short stalks; and in *Sergestes* the stalks are very long.

In *Lucifer* the spines over the eyes are absent; in *Acetes* they are present and simple; and in *Sergestes* they are very long and compound.

In *Lucifer* the postero-lateral spines are parallel to the long axis of the body; in *Acetes* they are oblique, so that they project a little beyond the outline of the body; and in *Sergestes* they are at right angles to the long axis, and compound.

The carapace, including the rostrum, makes about one-third of the total length of the body of the Lucifer *Zoëa;* about one-half of that of the Acetes *Zoëa;* and more than two-thirds of the total length of the Sergestes *Zoëa.* The abdominal somites of the Acetes *Zoëa* are shorter and wider than those of the Lucifer *Zoëa*, and this change is carried still further in the Sergestes *Zoëa.* In the Lucifer *Zoëa* the sixth abdominal somite is the only one which has ventral spines, and these point backwards. All the abdominal somites of *Acetes* have spines, and they point backwards and a little outwards, while in *Sergestes* they all point directly outwards.

The telson is slightly notched in *Lucifer;* deeply forked in *Acetes;* and in *Sergestes* the prongs of the fork diverge so much as to form a right angle.

These facts are extremely interesting, as they seem to show that the *Elaphocaris* is a larva essentially like that of *Lucifer*, which has passed through a remarkable process of secondary modification, resulting in the acceleration of the development of the eyes, and the production of a forked telson, and a very spiny body. The larva of *Acetes* has been modified in the same direction but to a much less degree. It may be asked why we are to assume that the Lucifer *Zoëa* is the primitive form, and the *Elaphocaris* larva the secondary modification rather than the reverse; but a little thought will show that the distinctive features of the *Elaphocaris* stand in direct relation to the environment, as weapons of defence, sense organs, or locomotor apparatus, while the distinctive marks of the Lucifer *Zoëa* are features of general or typical resemblance to the corresponding larva of *Euphausia* and *Penæus.*

I did not succeed in finding the *Protozoëa* from which the *Zoëa* shown in fig. 79 is derived, but I think it extremely probable that future research will show that an unknown larva which has been figured by DOHRN and CLAUS is the *Protozoëa* of *Acetes,* or else of a new closely-related genus of the Sergestidæ.

In his " Untersuchungen über Bau und Entwickelung der Anthropoden " (Zeit. f.

Wiss. Zool., xxi., 1871), DOHRN describes the "Larve eines unbekannten Krebses" from the Indian Ocean (p. 377), which is shown in his plates 29 and 30, figs. 62 to 67. In his 'Crustacean-System' (taf. iv., figs. 2 to 7) CLAUS gives much more satisfactory figures of what appears to be the same larva, and speaks of it as a "Phyllopodenähnlichen Protozoëa unbekannter Herkunft." Its close resemblance to the *Protozoëa* of *Lucifer* renders it extremely probable that it is the *Protozoëa* of a Sergestid, and as the *Protozoëa* of *Lucifer* and that of *Sergestes* are known, this must be the larva of *Acetes*, or of some closely-related unknown form.

The carapace is nearly smooth, rounded, and there is no trace of a rostrum, and it makes more than three-quarters of the total length of the body.

The compound eyes are present and well developed, but they are sessile, and there is no indication of the stalk. The first antenna is seven-jointed, and the two terminal joints are thin and long.

The second antenna is nearly twice as long as the first, and very thick. Its short stout basal portion consists of two joints, and carries a short two-jointed exopodite, with three long terminal non-plumose swimming hairs, and a very large twelve-jointed endopodite with a long swimming hair at each joint.

CLAUS's figures show that the appendages at the back of the antennæ are very much like those of *Lucifer*, and the same ones are present; that is, the mandibles, first and second maxillæ, and first and second maxillipeds.

The hind body is segmented, and ends in a broad, flat, deeply-cleft telson, with six pairs of irregularly plumose hairs, the third pair very much longer and thicker than the others.

A comparison of CLAUS's figure with fig. 27 of this paper will show that most of the differences between this unknown larva and the first *Protozoëa* of *Lucifer* are of the same kind as the differences between the Acetes *Zoëa* (fig. 79) and the corresponding stage of *Lucifer* (fig. 44).

At a time when the eyes of *Lucifer* are rudimentary and sessile they are perfect and stalked in *Acetes*, and at a time when they are entirely absent in *Lucifer* DOHRN's larva has them sessile and rudimentary but distinct.

The *Zoëa* of *Acetes*, like this larva, has its telson deeply forked; its hairs are plumose, and the third is much longer than the others. These resemblances, and the great length of the carapace, render it very probable that this unknown larva is the *Protozoëa* of *Acetes*.

I will now continue my description of the appendages of the *Zoëa*.

The first antenna (fig. 77, *A*) is uniramous, and it consists of a long, cylindrical, two-jointed shaft, and a single short flagellum, which shows obscure traces of a division into three joints. The basal joint of the shaft is a little more than half as long as the second joint, and it carries a single short sharp hair on the inner side of its distal end. The second joint has two much longer hairs on its distal end, and one about half way between its ends. The flagellum makes about one-fifth of the total length of the appendage, and it carries four terminal hairs, two of them about as long as those on the terminal joint of the shaft, and two nearly three times as long.

The second antenna is the chief locomotor organ, and (as shown in fig. 77, *An*) it consists of a thick two-jointed basal portion, which carries a two-jointed exopodite (*ex*) and a ten-jointed endopodite (*en*).

The proximal joint of the exopodite is about twice as long as the terminal joint, and it carries two long hairs on its outer end, and two more near the base. The terminal joint has, at its tip, one short hair, and four which are about as long as the limb. The endopodite consists of four short rings, and a series of six joints like those of the corresponding organ of the *Nauplius*, *Protozoëa*, and *Zoëa* of *Lucifer*. The terminal joint carries four, and each of the five other joints one long swimming hair, and none of these hairs are plumose.

On the basal portion of the appendage there is a large bright-red pigment-spot, which forks and runs along the exopodite and endopodite, about half way to their tips.

The labrum (fig. 77, *L*). is smaller than that of *Lucifer*, with a spine and a large red pigment-spot.

The mandible (fig. 77, *M*, and fig. 80) has small irregular denticles along its cutting edge, and these reach to the tip of the long tooth which occupies the posterior angle of the blade. The mandibles of two specimens were dissected out, and in each case there was a little hairy pad (*m*) upon the posterior surface. It could also be seen in the entire animal (as shown in fig. 77). It is possible that this pad is the mandibular palpus, but it seems much more probable that it is half of the lower lip or metastoma, for no palpus is present on the mandible of *Lucifer*.

The first maxilla (fig. 77, *Mx.* 1, and fig. 81) is quite different from that of *Lucifer* (fig. 46) at the same stage, but the difference is in minor points, and there is essential agreement in general structure. The two basal joints or blades are long and slender, and their hairs are also longer and thinner than they are in *Lucifer*. The endopodite (*en*) is placed nearly at right angles to the base, and is distinctly three-jointed. It carries five hairs as it does in *Lucifer*, and they are similarly placed, but longer. The three hairs on the scaphognathite are about equal in length, and the plumules on their sides are short and irregular.

The second maxilla (fig. 77, *Mx.* 2, and fig. 82) is much like that of the Lucifer *Zoëa* (fig. 47), but the three hairs at the tip are more than twice as long as those on the inner edge of the appendage, and they are irregularly plumose, while they are simple in *Lucifer*.

The first maxilliped (fig. 77, *Mp.* 1, and fig. 83) differs from that of *Lucifer* (fig. 48) in the same way, and the exopodite carries seven instead of four hairs, and these are as long as the appendage, and two-jointed.

The second maxilliped (fig. 77, *Mp.* 2) is about as long as the first, but it does not seem to be of much functional importance. It is usually carried stretched back along the hind body, as shown in the figure, and its hairs are short. As in *Lucifer* at the same stage, the exopodite is as long as the endopodite.

The third pair of maxillipeds, and the first, second, third, and fourth pairs of thoracic limbs are represented by buds, as in *Lucifer* at the same stage. The bud for the third maxilliped (fig. 77, *Mp.* 3) is bilobed, longer than the others, and it points backwards outside the other buds. The buds for the first three pairs of periopods are bilobed, in contact on the median line, and about equal in size. Those for the fourth pair are much smaller, and are hidden in a ventral view by the buds for the third pair, but they can be seen in side view (as shown at *T* 4 in fig. 78). There is no trace of the fifth pair of pereipods either at this or at any later stage. CLAUS figures buds for the fifth pair in the *Zoëa* of *Sergestes*, and also in the next or *Acanthosoma* stage of *Sergestes;* but the study of the *Zoëa* of *Acetes* shows even more satisfactorily than is the case in *Lucifer* that these appendages are entirely absent, and it seems safe to believe that this is the case in *Sergestes* also until the larva of the latter has been carefully examined with reference to this particular point.

The abdominal appendages, with the exception of the fifth pair, are entirely absent; but each abdominal somite has a pair of long ventral spines. The swimmerets are represented by long bilobed buds, which project beyond the fork or notch in the telson. The abdominal ganglia are very much more conspicuous than they are in *Lucifer*.

The distribution of pigment is somewhat different from what we find in the Lucifer *Zoëa*, and nearly all the pigment-spots are bright-red. There is a large spot of red and one of reddish-yellow on the eye stalk, a red spot on the labrum, a large red and very dendritic spot on the second antenna, red spots on the dorsal surface of the posterior edge of the third, fourth, and fifth abdominal somites on the median line; red spots on the ventral surface of the first, second, and third at the bases of the spines; a red and a brown spot at the base of the spine on the fifth; a brown spot at the base of the spine on the sixth, and a red spot on the base of the swimmeret. The anal spots are large and bright-red.

On September 20th I found several specimens of the stage which has just been described. Fig. 79 was made from one of them, which was then placed in a glass of water by itself, and the next day it was found to be moulting. In the evening the moult was found to be finished, and the larva was swimming actively. The drawing given in fig. 84 was made from it without injuring it, and later stages were also drawn from the same specimen.

The larva, $\frac{70}{1000}$ inch long, has undergone very great change, and although it is an *Acanthosoma*, it presents many important differences from both *Lucifer* and *Sergestes*.

The abdomen has lengthened so that the carapace makes less than one-third the total length of the body, and the dorsal and postero-lateral spines have disappeared.

The abdominal spines stand out from the body, and the swimmerets have become the chief locomotor organs. The spine has disappeared from the labrum; the two pairs of antennæ have changed from the larval to the adult form; the endopodite of the fourth pereiopod, and the first three pairs of pleopods are represented by long buds.

The first antenna (fig. 84, *A*) consists of a three-jointed shaft about as long as the carapace, and two terminal flagella. The basal joint of the shaft makes half the total length of the appendage, and the other two are about equal to each other. On the inner edge of the distal two-thirds of the shaft there are eight long, similar equidistant, plumose hairs, and there are two short spines on the outer edge. The inner flagellum is short, and carries one long slender terminal hair. The outer one is more than twice as long, and carries two thick sensory hairs.

The exopodite of the second antenna (fig. 84, *ex*) has become a scale, only one-third as long as the endopodite, which is now a ten-jointed flagellum about as long as the carapace.

The second and third maxillipeds (fig. 84, *Mp.* 2 and *Mp.* 3) and the first, second and third pereipods (fig. 84, *T* 1, *T* 2, and *T* 3), are Schizoped-like, but they are of very slight functional importance, and their endopodites are folded forwards on the ventral surface, like the maxillipeds of *Squilla*, so that it is impossible to study the mouth parts without dissection. The endopodite of the fourth pereiopod has entirely disappeared, and the limb is represented only by its exopodite. The five exopodites are about alike, and they all end in long slender swimming hairs: those of the four pereiopods (*T* 1 *ex*, *T* 2 *ex*, *T* 3 *ex*, and *T* 4 *ex*) are bent outwards and upwards towards the dorsal surface, as in the maxillipeds of a Crab *Zoëa*, but those of the second and third maxillipeds (*Mp.* 2 *ex* and *Mp.* 3 *ex*) are more nearly parallel to the endopodites. The endopodite of the second maxilliped (*Mp.* 2) is free and movable, but those of the third maxillipeds (*Mp.* 3) and of the first, second, and third pereiopods are covered by a delicate cuticle, and are almost immovable.

I did not actually witness the next moult, but four days later the larva, $\frac{80}{1000}$ inch long, was in the stage shown in fig. 85. The exopodites of the thoracic limbs have become reduced to rudiments, the limbs themselves have stretched out and are now functional, as are the three pairs of abdominal feet.

The first antenna (fig. 86) has not changed much, but its base is swollen and the otocyst has appeared.

The second antenna is now half as long as the whole body, its flagellum is ten-jointed, and red pigment has appeared at its base and tip (fig. 87). The outer end of the scale carries nine long plumose hairs arranged on the tip and inner edge.

The second maxilliped (fig. 88, *Mp.* 2) is bent into a knee, and is fringed by sixteen plumose hairs. Its exopodite is rudimentary, but longer than in any of the appendages which follow.

The third maxilliped (fig. 88, *Mp.* 3) is long, slender, six-jointed, with a rudimentary endopodite.

The other three limbs (fig. 88, *T* 1, *T* 2, *T* 3) are six-jointed and they end in enlarged chelæ. The first is the shortest; the second is about as long as the third maxilliped, and the third is still longer. The fourth is now represented only by a small rudiment and a ganglion; and the fifth is entirely absent, as it has been at all stages.

The three pairs of pleopods are alike in structure, and each consists of two joints about equal in length (fig. 89). The outer half of the terminal joint is toothed and carries six pairs of long slender non-plumose hairs, so arranged as to form a paddle.

The rostrum (fig. 85) is long and curved, and it has a single secondary spine in its upper surface. A pair of very small spines have also appeared at its base.

This specimen had nearly completed its moult into the stage shown in fig. 90 on the last day of my stay at the seashore—five days after fig. 85 was drawn.

Fig. 90 was drawn from another specimen, $\frac{15}{100}$ inch long, which was captured at the surface on September 25th.

The eye-stalks are long and very movable, the flagellum of the second antenna is considerably longer than the body, the five pairs of thoracic limbs have developed gills, and the fourth and fifth pleopods have appeared; but in other respects the structure is nearly as it was in the preceding stage. The endopodites of the maxillipeds are pointed, but those of the three pereiopods end in rudimentary chelæ. The endopodite of the third pereiopod is much longer and thicker than the others, and its tip reaches nearly to the rostrum.

The buds for the first three pairs of pleopods are long, obscurely jointed, and they meet each other on the median line. There are as yet no traces of the fourth and fifth pairs. The spines on the abdominal somites are long and sharp. Those on the first three somites point outwards and forwards, those on the fourth point almost directly outwards, and those on the fifth outwards and backwards. The sixth abdominal somite has lengthened, and is now about as long as those of the others. The telson is short and shield-shaped, with two pairs of long and one pair of very short spines, and the swimmerets are perfectly formed and fringed with long plumose swimming hairs.

The exopodite is long, narrow, with a smooth outer edge which ends in a tooth, and a rounded point. It carries fifteen hairs: ten on the inner edge, two on the tip, and three between the tip and the tooth. The endopodite is nearly as long and wide as the exopodite, and it carries nineteen hairs: two at the end, eight on the outer, and nine on the inner edge.

The ocellus is still present, and the pigment-spots have nearly the same arrangement as before, but some of them are now yellow or green instead of red.

The eye-stalk is about as it was before, and the ocellus is still present and double. As regards the more minute structure of the appendages the first antennæ are now about as long as the carapace, and most of the increased length is in the flagellum, which now consists of seven joints. The secondary flagellum is still quite short. The shaft of the antenna is three-jointed, as before, but the basal joint is much lengthened, and now makes more than half the total length. The auditory organ at its base is now very conspicuous, and the inner edge of the shaft carries eleven hairs instead of six; five of these are on the basal joint, three on the second joint, and three on the third.

The scale of the second antenna has lengthened and is now more than half as long as the first antenna, including the flagellum. Its inner end carries eight, and its tip three hairs, and the outer edge of the tip is toothed. The swollen base of the flagellum of the second antenna carries a large red pigment-spot, and the flagellum, which is considerably longer than the body of the animal, is also marked by bright-red pigment throughout the greater part of its length.

The second maxilliped has become completely bent upon itself, and it bears a close resemblance to that of the adult *Lucifer*, although it carries a gill, as do the second maxillipeds and the three pairs of thoracic limbs. All traces of the exopodites have disappeared from all these appendages, but their structure and comparative length are about as before.

The first, second, and third pairs of abdominal feet have increased in length, and the first is now almost as long as the last thoracic limb. The second is a little shorter; the third is still shorter, and has acquired a second terminal branch, which is as yet rudimentary.

The fourth and fifth pleopods, which have now made their appearance, are much smaller than the others, and each has one large and one small terminal branch.

The swimmerets and telson are very similar to those of the immature *Lucifer*, although the telson is shorter and wider. The exopodite has fifteen hairs on its inner edge, two on its rounded tip, and four between the tip and the tooth. The endopodite has nineteen hairs. The surface of the carapace is finely punctated, and the rostrum has no secondary spine. The spine has disappeared from the first abdominal somite, and the one on the third somite is longer than any of the others. The dorsal surface of the third somite is bent, so that the abdomen is no longer perfectly straight. Large conspicuous red pigment-spots have appeared on the lower edges of the second, third, fourth, and fifth abdominal somites.

As the series of drawings which I have given was made from such a small number of specimens, I am unable to contribute much information as to the changes of the mouth parts, and must leave this, as well as the exact determination of the adult form and systematic position of the species, to future research.

In his 'Facts for Darwin' FR. MÜLLER has figured a larva (fig. 33) which is extremely like, if not identical with the one shown in fig. 90, and he regards it as the young of a Prawn, closely related to *Penæus*. CLAUS has suggested ('Crustacean-System,' p. 35) that it is much more likely to prove to be a young Sergestid than a Prawn, and the facts regarding its metamorphosis which I have given above, certainly seem to point in the same direction. An earlier stage of development is given in FR. MÜLLER's fig. 32, and a comparison with my fig. 84 will show that the same larva at an earlier stage might, when crushed by a cover glass, present very much the same appearance as this larva. If they are the same FR. MÜLLER is certainly mistaken in his statement that fig. 33 follows directly after fig. 32, without the intervention of a Schizopod stage, for the metamorphosis is really quite complicated, and a true Schizopod stage exists, although it is of extremely short duration.

VI.—RELATION BETWEEN THE LARVÆ OF LUCIFER, ACETES, SERGESTES, PENÆUS, AND EUPHAUSIA, AND THE SIGNIFICANCE OF THE DECAPOD ZOËA AND THE CRUSTACEAN NAUPLIUS.

The general significance of the peculiar type of Decapod metamorphosis, of which *Lucifer* is now the most thoroughly known illustration, has been discussed with the greatest ability and knowledge of the facts by CLAUS in his 'Untersuchungen zur Erforschung der Genealogischen Grundlage des Crustacean-Systems.' My own acquaintance with the phenomena of Crustacean morphology in general is very far from being sufficiently extended and minute to qualify me for a critical discussion of this work; but while the facts in the life-history of *Lucifer* seem to tend to a similar conclusion, and even to place it upon a much firmer basis than before, they also indicate that CLAUS'S views cannot receive unqualified acceptance in their present shape.

I shall not venture at present upon the broader aspects of the question, but I wish to draw attention to the resemblances and differences between the various larval stages of *Lucifer* and those of a few closely-related forms. The materials which are at present available for a comparison of this kind are extremely scanty, for there is no other closely-related form in which all stages, from the egg to the adult, have been actually traced in a single species by rearing captive specimens.

Comparison of Lucifer *and* Acetes.

The genus which shows the closest similarity to *Lucifer* is *Acetes*, but in this case we are ignorant of both the early and the later stages. During the last "*Zoëa*" stage the resemblance between the two forms is well marked, and is shown in such features as the similarity in the shape of the carapace and hind body; in the length and structure of the two pairs of antennæ; in the mode of locomotion; by rowing with the antennæ; in the presence of an ocellus; the presence of a spine on the labrum; the close similarity of the mouth parts and maxillipeds; the rudimentary structure of the thoracic limbs and swimmerets; the total absence of the fifth thoracic somite; and the absence of the first five pairs of pereiopods. Notwithstanding these resemblances the differences are quite conspicuous. The eye is sessile in *Lucifer*, stalked in *Acetes*. The shaft of the first antenna is one-jointed in *Lucifer*, two-jointed in *Acetes*. The endopodite of the second antenna has two basal rings in *Lucifer*, four in *Acetes*. The two lobes of the metastoma are conspicuous in *Acetes*, and could not be made out at all in *Lucifer*. The abdominal somites are rounded in *Lucifer*, and spiny in *Acetes;* and the telson is deeply forked in the latter, slightly notched in the former. In a word, the resemblances between the two are general rather than detailed, and the differences are specific differences of the same character as those between closely related adult animals. A comparison of column 2 of Table V. with column 1 will show these resemblances and differences in tabular form.

If we assume the correctness of the extremely probable assumption that DOHRN's and CLAUS's unknown larva is the earliest *Protozoëa* of *Acetes*, the resemblances between it and the corresponding larva of *Lucifer* (compare fig. 27 with CLAUS, fig. 2, taf. iv.) are much greater than they are at a later stage. The chief differences are the presence in *Acetes* of rudimentary compound eyes; the great length of the carapace; the absence of a rostrum and spines; the great number of joints in the first and second antenna, and the difference in the length of these two appendages; the deep notch in the telson. The close similarity between the two larvæ at this stage will be seen by comparing column 1 of Table IV. with column 2.

After the moult which ends the *Zoëa* series the differences between the *Acetes* larva (fig. 89) and the *Lucifer* larva (fig. 53) become much greater, although they do not obscure the fundamental similarity between the two forms. In each of them the carapace makes less than one-third the total length of the body, and it has a rostrum and two antero-lateral, but no postero-lateral or dorsal spines. The first antenna has lost its swimming hairs, and has developed one flagellum in each form and two in *Acetes.* In both forms a series of long plumose hairs has appeared on the inner edge of the shaft of the appendage. In both forms the second antenna has lost its locomotor function and assumed the adult form, but it is rudimentary in *Lucifer* and well developed in *Acetes.*

The ocellus is present and the eye stalked and movable in both.

The fifth thoracic somite and its appendages are entirely wanting in both forms. The fourth is biramous in *Lucifer*, and similar to the ones before it, but in *Acetes* the limb proper has disappeared and the appendage is represented only by an exopodite. The second and third pairs of maxillipeds, and the first, second, and third pairs of pereiopods are essentially alike in structure in both forms, but in *Acetes* the endopodites are rudimentary, covered by a cuticle, and functionless. The swimmerets are present and very similar in the two forms, but the other abdominal appendages are absent in *Lucifer*, while the first, second, and third pairs are developed, but rudimentary in *Acetes.* The abdominal somites have acquired ventral spines in both forms, but these are very small in *Lucifer* and long and prominent in *Acetes.* The telson is long and narrow in *Lucifer* and short and wide in *Acetes.* The relation between the two forms at this stage of development will be seen by a comparison of columns 1 and 2 of Table VI.

The later history of the two genera can hardly be divided into parallel stages. *Lucifer* keeps all its Schizopod limbs for at least two more moults, and as shown in fig. 54, acquires the rudiments of all the abdominal feet at one time, and before the fourth pair of thoracic limbs and the exopodites of the others and of the maxillipeds disappear, while *Acetes* (fig. 85) loses its exopodites at once, and the maxillipeds, thoracic limbs, and antennæ become like those of an adult Sergestid some time before the appearance of the five pairs of pleopods; and these do not appear together, but in two sets.

It is interesting to note that although the changes which the two forms undergo at successive moults do not admit of exact comparison with each other; the outcome, after a few moults, is almost exactly the same, as will be seen by a comparison of fig. 60 with fig. 90.

The number and character of the somites and appendages is now the same, and while the two forms differ greatly in outline and proportion, the young *Acetes* is essentially like the young *Lucifer*, except in the length of the flagellum of the second antenna, the presence of chelæ on the thoracic limbs, the presence of gills, and the absence of a "neck." The outcome of the process of development is alike, but the paths followed diverge from each other to converge again at this stage.

Comparison of Lucifer *and* Sergestes.

The metamorphosis of *Sergestes* is more like that of *Lucifer* than is the case with any other known Crustacean except *Acetes*, but our knowledge of the development of *Sergestes* is incomplete, and we have no assurance that the various stages which have been described belong to the same species.

In 1870, DOHRN described a remarkable larva ("Untersuchungen über Bau und Entwickelung der Decapoden, No. 10, Beiträge zur Kenntniss der Malacostraken und ihrer Larven, Part 4, Beschreibung einer neuen Decapoden-Larve," Zeit. f. Wiss. Zool., xx., p. 607) which he collected at the surface at Messina, and which he was unable to refer to any adult form. He proposed for this larva the provisional name *Elaphocaris*. *Elaphocaris* is a *Zoëa* which so far as its appendages are concerned does not differ much from the last *Zoëa* of *Lucifer*, but its abdomen is very spiny, and the spines on the carapace are drawn out so that each one of them is nearly half as long as the body, and they are fringed with rows of long secondary spines which are hooked at their tips, and so arranged as to give to the body a very grotesque appearance, and the larva does not, at first sight, show any similarity to the simple *Erichthina* larva of *Lucifer*.

CLAUS had several years before described ("Ueber einige Schizopoden und niedere Malacostraken Messinas," Zeit. f. Wiss. Zool., xiii., 1863) a larval Crustacean with swimmerets, biramous thoracic limbs, and a very spiny body, which he calls an *Acanthosoma*. This same larva, or a very closely related form, had been figured and described nearly twenty-five years before by DANA ('Crustacea,' p. 664, plate 44, fig. 5) as *Sceletina armata*.

In the same paper CLAUS gives a figure of a young Crustacean, which had previously been described by LEUCKART under the name of *Mastigopus*, and shows that it is in all probability a young *Sergestes*.

In his 'Untersuchungen zur Erforschung,' &c., he describes an *Elaphocaris* at a much younger stage than DOHRN's figure, and shows that this larva, DOHRN's *Elaphocaris*, his own *Acanthosoma*, and LEUCKART's *Mastigopus* are successive stages in the development of *Sergestes*.

From independent researches in the South Pacific, WILLEMÖES-SUHM also ascertained (Proc. Royal Soc., Dec. 9, 1875, p. 133) that *Elaphocaris* is the larva of *Sergestes*, and he traced its development through the *Acanthosoma* stage, which, from its resemblance to *Amphion*, he calls the *Amphion* stage.

These various observations, and especially those by CLAUS, give us a pretty complete acquaintance with the metamorphosis of *Sergestes* from the first *Protozoëa* stage to maturity.

The first *Protozoëa* (CLAUS, 'Untersuchungen,' taf. v., fig. 1) has, like the *Protozoëa* of *Lucifer*, locomotor antennæ, a spine on the labrum, a partially segmented hind body, and a very spiny telson. The mandibles, first and second maxillæ, and first and second maxillipeds are like those of the corresponding *Lucifer* larva. In addition to the spiny carapace it presents the following conspicuous differences from the *Lucifer* larva. The eyes are stalked, movable, and compound. The first antenna has seven joints. The endopodite of the second antenna has no small rings at its base. There is a third pair of maxillipeds. Five thoracic somites are represented in the figure. The telson is very deeply cleft. The relation between the larva and the first *Protozoëa* of *Lucifer* will be seen by a comparison of columns 1 and 3 of Table IV.

The next stage which CLAUS describes (taf. vi., fig. 1) is no doubt separated from the first by one or more intermediate stages. The rostrum has developed a pair of long secondary compound spines at its base, which do not correspond to anything in the corresponding larvæ of *Acetes* and *Lucifer*.

The thoracic limbs are represented by *five* pairs of rudimentary bilobed buds. There are five free abdominal somites without appendages, and the sixth and telson are represented by an unsegmented region, which carries a pair of long bilobed pouches, the rudimentary swimmerets.

The relation between *Elaphocaris* and the corresponding larvæ of *Acetes* and *Lucifer* may be understood by a comparison of column 3 of Table V. with columns 1 and 2.

In the next or *Acanthosoma* stage ('Untersuchungen,' taf. v., fig. 6) the two pairs of antennæ assume the adult form, and the thoracic limbs and swimmerets become developed as they do in *Lucifer*, and the carapace loses its posterior spines, although there are three in place of one pair of anterior spines. The telson is distinct from the last abdominal somite, and all the abdominal somites have projecting spines.

The eye-stalks are much longer than they are in the other two forms. The first antenna has a secondary flagellum, as in *Acetes*, and the scale and flagellum of the second antenna are well developed.

The exopodites of the maxillipeds and pereiopods are very long, many-jointed, except in the first maxilliped, and they are longer than the endopodites in all the pereiopods.

The fifth pair of pereiopods are present and like the others. The swimmerets are very long and slender, and the telson very short and forked.

This stage, like the corresponding stage of *Lucifer*, and unlike that of *Acetes*, persists for more than one moult, and the five pleopods make their appearance together, as rudimentary buds, before the exopodites of the pereiopods and maxillipeds disappear.

The third column of Table VI. shows the resemblances to *Lucifer* and *Acetes* at this stage.

In the immature or *Mastigopus* stage (see CLAUS's 'Ueber einige Schizopoden und niedere Malacostraken, Messinas') the three forms are almost exactly alike, except as far as the generic distinctions are concerned, and the young *Sergestes* scarcely differs from the young *Lucifer* except in the absence of a neck, the length of the flagellum of the second antenna, and the presence of rudiments of the fourth and fifth pairs of pereiopods.

Comparing the whole course of development of the three forms, as far as it is known, we notice that while the larval stages of *Sergestes* are much more different than those of *Acetes* from the corresponding stages of *Lucifer*, the character of the change at each moult is much more like what we have in *Lucifer* than what we have in *Acetes*.

We cannot fail to notice, in the second place, that the attempt to express the facts of the metamorphosis of these forms, so far as we know them, in a tree-like diagram, would result in a tree placed upside down, with the branches which represent the three Protozoëas much more divergent than those which represent the three young Sergestids. A similarity of type runs through the whole metamorphosis, but it is no more marked at the early stages than it is in the late stages, while the secondary differences are much more conspicuous during the *Zoëa* and *Acanthosoma* stages than they are as we approach the adult form.

While this is true it is also true that if we imagine a metamorphosis which shall agree with these three in all their common features, but shall have none of the features which they do not all share, we shall have something much more like the metamorphosis of *Lucifer* than that of *Acetes* or *Sergestes*, and we must therefore regard the life-histories of these three forms as somewhat divergent modifications of a form of development which is at present more closely adhered to by *Lucifer* than by the other two, and in this metamorphosis we must recognise a *Protozoëa* stage when the two pairs of antennæ are locomotor, the ocellus present, the labrum furnished with a spine, the carapace armed with posterior dorsal and lateral spines and a rostrum; the two pairs of maxillæ, and two pairs of maxillipeds present, and the thoracic and abdominal segments without appendages. This stage persists, with slight modification, through several moults in all of them, and is followed by an *Acanthosoma* stage, in which the carapace has a rostrum and antero-lateral spines, and a smooth posterior edge; the eyes are stalked; the two pairs of antennæ have their adult character; there are at least four pairs of pereiopods with swimming exopodites; the swimmerets are large and have their adult form, and the other abdominal appendages are absent. The duration of this stage and the mode of transition to the next varies in the three forms, but it is followed in all by what may be called a *Mastigopus* stage, characterised by the general features of the family.

In all three forms the somites, and with the exception of the swimmerets the appendages also, develop in serial order from in front backwards.

The interesting question whether we are to attribute to this typical form of development a fifth thoracic somite and appendages must, I think, be left in doubt. A comparison of the Sergestid larvæ seems to indicate its absence, but wider comparison with *Penæus* and the Schizopods seems to lead to the opposite view.

Comparison of Penæus *with the Sergestidæ.*

In order to render the comparative tables as complete as possible, I have added columns showing the corresponding stages of *Penæus* and *Euphausia.*

FRITZ MÜLLER has described a number of stages in the development of a species of *Penæus* ("Verwandlung der Garneelen," Arch. f. Naturgeschichte, 1863, pp. 8–23, taf. ii.). The series commences with a *Nauplius* which may belong to the same species, although we have no certainty of this. In a second paper ("Ueber die Naupliusbrut der Garneelen," Zeit. f. Wiss. Zool., xxx., 163–166) he gives, in reply to doubts which had been expressed to him by SPENCE BATE, ALEX. AGASSIZ, PAUL MAYER, and others, the following reasons for believing in the specific identity of all the forms in his series:—1st, the peculiar mode of locomotion; 2nd, the resemblance in colour; 3rd, the great length of both pairs of antennæ; 4th, the character of the mandible; 5th, the presence of four pairs of buds in the *Nauplius,* and four corresponding pairs of limbs in the *Zoëa;* 6th, the similarity in the structure of the heart, digestive tract, and liver in the *Nauplius* and the youngest *Zoëa;* 7th, the presence of frontal organs in both stages. As all the points except the colour would apply to any Crustacean which passes through a *Protozoëa* stage, there is certainly nothing more than a presumption that the whole of his series represents a single species; but as there is no doubt that the *Nauplius* belongs to *Penæus* or to some closely-related form, I have included it in the table.

FR. MÜLLER'S account of the later stages is supplemented by a few additional observations of other species by CLAUS ("Untersuchungen," &c., pp. 11 and 41, taf. ii. and iii.), and I have compiled the columns in the tables from both sources.

The first *Nauplius* stage (Table II., column 3) appears to be more simple than that of *Lucifer,* as MÜLLER failed to observe any buds to represent appendages posterior to the mandibles.

The *Nauplius* stage is followed by a meta-*Nauplius* stage (Table III., column 2), which is distinguished from that of *Lucifer* by the great size of the blade of the mandible, by the presence of frontal organs, and by the shortness of the carapace.

The next stage is a *Protozoëa* (Table IV., column 5), with a rounded carapace without spines or rostrum, four basal rings and six terminal joints in the endopodite of the second antenna, a spine on the labrum, two pairs of maxillæ, two pairs of maxillipeds, and a long hind body which, according to CLAUS, is divided into six thoracic and five abdominal somites, and terminates in a deeply-forked telson with seven pairs of spines.

This stage persists with slight change for several moults, and at the last the buds for the thoracic limbs and swimmerets appear. According to CLAUS, the rudiments of all the abdominal appendages can be seen at an earlier stage.

The passage from the last of the *Protozoëa* series to the first Schizopod stage is attended by a complete change in the structure of the antennæ, and these now assume the adult form. The carapace also acquires two antero-lateral spines and two more at the base of the rostrum. At this time it is much like *Lucifer*, as shown in column 4 of Table VI., but the endopodites of the third pair of maxillipeds and of the pereiopods are rudimentary, and shorter than the very long-jointed exopodites.

The significance of the various stages in the metamorphosis of the higher Crustacea is one of the most interesting questions in the whole field of morphological science, and it has given rise to at least its due share of speculation, but it will not be out of place to examine the relation between the facts which have been described and the various theoretical views which have been expressed upon the subject. In the case of the Sergestidæ it is obvious, in the first place, that the adult *Lucifer* and *Acetes* also, if *Acetes* be an adult, are little more than mature representations of the *Mastigopus* stage, complicated in the case of *Lucifer* by the formation of a neck, and in the case of *Acetes* by the presence of gills, and chelæ on the pereiopods. There can also be little doubt that the Schizopod stage of development in the Sergestidæ and *Penæus* bears a similar relation to the adult Schizopods, especially to *Amphion*, the adult character of which seems to be established by WILLEMÖES-SUHM'S observations (Proc. Roy. Soc., Dec. 9, 1875).

The significance of the *Zoëa* stage in the higher Decapods is one of the most vexed points in Crustacean morphology. We have shown that in the Sergestidæ and in *Penæus* the so-called *Zoëa* stage is nothing but a preparation in the *Protozoëa* for the next or Schizopod stage; that it involves no changes of structure except those which are related to the form which it is to assume after the next moult, and that the *Zoëa*, as a distinct stage, is absent. The life-history of these forms would therefore lead us to suspect that the Brachyuran *Zoëa* is a secondary modification of the more primitive *Protozoëa*, and we may perhaps see in the larval skin which many Crab-*Zoëas* shed soon after or even before they leave the egg, and which usually has a conspicuously forked and very spiny telson—a remnant of the unmodified *Protozoëa* stage.

DOHRN ('Geschichte des Krebsstammes, Jenaische Zeitschr.,' 1871) and FRITZ MÜLLER ('Für Darwin') have held that the typical *Zoëa*, with segmented abdomen and suppressed thorax, is the ontogenetic recapitulation of an ancestral form which has formerly existed as an adult, and DOHRN even goes so far as to recognise the still more remote ancestor of this *Zoëa* type in an embryo ("Untersuchungen über Bau und Entwickelung der Arthropoden; eine neue *Nauplius*-form: Archizœa gigas," Zeit. f. Wiss. Zool., xx., 597), which WILLEMÖES-SUHM has recently shown ("On the

Development of *Lepas fascicularis* and the '*Archizœa*' of Cirripedia," by R. von Willemöes-Suhm, Ph.D., Proc. Roy. Soc., Dec. 9, 1875, pp. 129–130) to be the *Nauplius* of a Barnacle, in all probability *Lepas australis*.

Claus, on the other hand, believes that the *Zoëa* has no such ancestral significance ("Untersuchungen," &c., p. 31). That it has been formed by secondary modification of the *Protozoëa*, and that the views of Müller and others, that the *Zoëa* presents a picture of the remote ancestor of the Malacostraca, is fundamentally erroneous; and not only this, but that the *Protozoëa* itself is the result of the extreme secondary modification of an ancestral form which Claus proposes to call an Urophyllopod, and which he believes to have had the following characteristics ("Untersuchungen," p. 23): A greatly developed shield-like carapace, produced by a fold of the integument in the region of the maxillæ, and probably armed with median and unpaired spines; two maxillary segments and appendages, eight somites of the mid-body with appendages, and six abdominal somites with swimmerets and telson; a many-chambered heart; compound eyes, probably stalked; a first antenna with sensory hairs; locomotor second antennæ, in which the exopodite was probably a scale; the mandible probably lacked a palpus; the metastoma was represented by a pair of paragnathi; the maxillæ had their basal joints modified for mastication, their endopodites reduced to a jointed palp, and the exopodite modified to form a scoop or scaphognathite for regulating the flow of the respiratory current under the carapace.

The following eight pairs of appendages were more like Schizopod feet, and each of them carried a basal gill-plate; the six pairs of abdominal appendages had large basal joints with two branches and gill-plates.

Claus believes that we may recognise in *Nebalia*, which has stalked eyes, a scale on the first antenna; only one long flagellum on the second antenna; a mandibular palp; a highly specialised, long jointed endopodite on the first maxilla; two long limb-like rami on the second maxilla; eight pairs of phyllopod-like thoracic limbs with jointed endopodite, flat, spiny exopodite and gill; six pairs of pleopods, the last two rudimentary; and a seventh somite between the sixth abdominal somite and the deeply-forked telson ("Ueber den Bau und die Systematische Stellung von Nebalia," Zeit. f. Wiss. Zool., xxii. p. 323–330), a very slight modification of this ancestral Urophyllopod.

He gives on pages 69–71 of his "Untersuchungen," &c., a long, minute, and extremely ingenious explanation of the way in which this Urophyllopod stage of development became converted by secondary modification into the Malacostracan *Protozoëa*, and afterwards, by still greater modification in the same direction, into the typical *Zoëa* of the higher Decapods.

The facts which have been detailed and tabulated with reference to the metamorphosis of the Sergestidæ and *Penæus* seem to substantiate at least a portion of this view, and to show that the typical *Zoëa* is a secondary modification of the *Protozoëa*;

but a comparison of these forms with the metamorphosis of *Euphausia*, upon which **Claus** lays especial emphasis, seems to demand a directly opposite interpretation.

If the *Zoëa* has been produced by a secondary modification of the *Protozoëa* we should expect to find the characteristics of the *Protozoëa* better preserved in the Schizopods than in the lower Decapods, and if we find in the Schizopods certain features of the typical *Zoëa*, which are absent in the *Protozoëa* of the lower Macroura, we can hardly accept without question the interpretation which sees, in secondary modification of the latter, the origin of the *Zoëa*. In *Euphausia* the somites appear in regular succession, from in front backwards, but the somites of the abdomen acquire appendages before the pereiopods appear, and there is a stage when the abdomen is fully developed and the thorax almost absent; a stage which, therefore, resembles the Brachyuran *Zoëa* more perfectly than any stage in the development of *Lucifer*, *Acetes*, *Sergestes*, or *Penæus*.

We have no complete history of any one species of *Euphausia*, but the observations of **Metschnickoff** (Zeit. f. Wiss. Zool., xix., pp. 479–481, and xxi., pp. 390–401), and **Claus** (Zeit. f. Wiss. Zool., xiii., pp. 442–454, and "Untersuchungen," &c., pp. 9 and 33) give us a tolerably complete account of the metamorphosis of the genus.

Metschnickoff's larva is extremely like that of *Lucifer*, although there are many differences. It is interesting to note that it leaves the egg in a much more rudimentary form, passes through a greater number of moults, and attains to much greater structural complexity than *Lucifer* during the *Nauplius* stage. We can select three stages which agree pretty closely with the egg *Nauplius*, the first free *Nauplius* and the last, or meta-*Nauplius*, of *Lucifer*, but between, after, and before these stages there are others which are not found in *Lucifer*.

The youngest *Nauplius* (Zeit. Zool. xxi., fig. 2) is so much less advanced than the egg *Nauplius* of *Lucifer* six hours before hatching, that it does not seem probable that it normally leaves the egg in this condition.

It has an oval body, without ocellus, mouth, or labrum, and there is no trace of more than three pairs of appendages or of the carapace. At the next stage the swimming hairs of the first three pairs of appendages are fully developed, and the anus, notch, and two spines of the telson are present. In these respects it is more advanced, but in the rudimentary condition of the labrum and metastoma less advanced, than the first free *Nauplius* of *Lucifer*. The buds for the first and second maxillæ and the first pair of maxillipeds are present, but continuous across the median line of the body. According to **Metschnickoff**, the larva shown in fig. 3 of his first paper is in the next stage of development; but I can scarcely believe that it belongs to the same species, for the ocellus is absent, and the hairs on the three pairs of locomotor appendages are much more rudimentary than they are in fig. 3 of the second pupa.

The next stage (fig. 4) of the second paper agrees with the first free *Nauplius* of

Lucifer, so far as the form and number of the appendages is concerned; but the last pair of buds are biramous, and the carapace and telson are well developed. The next stage (fig. 5) of the second paper is more advanced in nearly every respect than the second free *Nauplius* or meta-*Nauplius* of *Lucifer*. The mandible is rudimentary, but still bilobed, with no trace of a blade. The outline of the carapace is free from the body, and its anterior and posterior edges are spiny. It has frontal organs, and the basal joint of the second antenna carries five recurved hooks.

According to the author, figs. 2 and 3 of the first paper show the next stage; but the structure of the hairs on the antennæ, the fact that they are plumose, and the very deep notch in the telson, seem to indicate that this is another species. However this may be, the structural complexity at this and the next (first paper, fig. 6) stage is much greater than we find it in *Lucifer* at the end of the *Nauplius* series.

It will be observed that, while METSCHNICKOFF'S larva and the *Nauplius* of *Lucifer* are essentially alike, there is at no time an actual agreement, since certain structures, as the carapace, become developed earlier, and others, as the labrum, later than they do in *Lucifer;* and certain structures, as the frontal organs and the hairs on the base of the antennæ, are entirely absent in *Lucifer*.

In column 4 of Table II. I have compared fig. 4 of METSCHNICKOFF'S second paper with the first free *Nauplius* of *Lucifer*, and in column 3 of Table III. his fig. 5 with the last *Nauplius* stage of *Lucifer*.

The various *Protozoëa* stages are shown by CLAUS in plate 1 of the "Untersuchungen," &c. The early *Protozoëa* (Table IV., column 5) is much like that of *Lucifer*, but the carapace is serrated, there is only one pair of maxillipeds, and, according to CLAUS there is a fifth thoracic somite. In the last *Zoëa* stage (Table V., column 5) all the abdominal somites and the rudimentary swimmerets are present, but there is no trace of the second and third pairs of maxillipeds or of the pereiopods.

Up to this point the course of development has followed essentially the same line as in the *Sergestidæ*, but, as we should expect, the *Protozoëa* series is not followed by a larval Schizopod stage, but by a series of moults during which the adult characteristics are gradually acquired. In the loss of the posterior spine of the carapace, the acquisition of antero-lateral spines, and the change in the antennæ from the *Nauplius* form to the adult form, the moult is like that of *Penæus* and the Sergestidæ; but the second and third maxillipeds and the pereiopods appear one at a time in succession from in front backwards, and the abdominal feet appear before the pereiopods. There is no *Zoëa* stage it is true, but the course of development differs from that of *Penæus* and the Sergestidæ in the very feature in which the larvæ of these forms differ from a typical *Zoëa*—the irregular manner in which the pereiopods appear.

I am therefore unable to give CLAUS'S interpretation of the significance of these larvæ unqualified acceptance at present, and feel that our groundwork in this department of knowledge can be made sure only by new observations. Every naturalist who can trace the whole life-history of a single species of any of the genera of lower

Malacostraca by actual moults, will not only help us to a sound and thorough appreciation of the significance of Crustacean embryology, but will also contribute to a better knowledge of the relation between ontogeny and phylogeny in the whole province of biology.

The phylogenetic significance of the *Nauplius* stage of development seems to me to rest upon a much firmer basis, and there are many reasons for believing that this is really an ancestral form. Its occurrence in so many widely-separated groups of Crustacea shows its great antiquity, and if it does not represent the adult form of the ancestral Crustacea, but a later larval form which has been produced by secondary modification of the original course of development, this secondary modification must have taken place very early in the history of the group, at a time when the adult forms were very primitive and unspecialised. A sufficient difference between the habits and surroundings of a young animal and those of the adult to favour secondary modification of the young is much less probable in an early unspecialised form, with simple habits, than it is in later and higher forms; and the older a larval form can be shown to be, the more probable does it become that it at one time existed as an adult.

The great age of the *Nauplius* stage and its definite structure therefore indicate that it is ancestral, and nothing except the supposed necessity for believing that the primitive Crustacean had a great number of somites and appendages seems to oppose this view.

I shall try to show further on that the serial homology shown by the parts of the body of one of the higher Crustacea cannot be fully accounted for by assuming, with BALFOUR ('Comparative Embryology,' p. 418), that the primitive Crustacean had, in addition to its three pairs of appendages similar to those of existing Nauplii, a long segmented body with simple biramous appendages; and I shall also try to show that this homology can be accounted for without any such supposition, so that the peculiarities which BALFOUR points out—1st, that the mandibles have the form of biramous swimming feet; 2nd, that the second pair of antennæ are biramous swimming feet; 3rd, that the body shows no traces of segmentation; 4th, that the heart is absent; 5th, that the ocellus is the sole organ of vision—must be allowed their full weight, and must not be opposed by any *à priori* assumption of the theoretical need for a greater number of somites and appendages.

TABLE II.—Comparative Table of the Parts of the Nauplii of *Lucifer*, *Penæus*, *Euphausia*, *Apus*, and *Palæmonetes*.

	First free Nauplius of *Lucifer*, [illegible] inch long (Plate 2, fig. 25).	First free Nauplius of *Penæus* (MÜLLER, "Verw. der Garneelen," fig. 1).	Free Nauplius of *Euphausia* (METSCHNIKOFF, Zeit. Zool., xxi., taf. xxxiv., fig. 4).	Free Nauplius of *Apus* (BALFOUR, fig. 208 from CLAUS).	Egg Nauplius of *Palæmonetes* (FAXON).
Compound eyes	Absent	Same	Same	Same	Rudimentary
Ocellus	Present	Same	Same	Same	Not observed until later
Frontal organ	Absent	Same	Same	Absent	Absent
Carapace	Absent	Same	Present	Indicated by fold	Absent
First antennæ	Uniramous, unjointed, locomotor	Same	Uniramous, obscurely jointed, as long as second, locomotor	Same, but shorter than second	Rudimentary, uniramous
Second antennæ	Biramous, unjointed, locomotor, no blade	Same	Biramous, jointed, locomotor, no blade	Biramous, unjointed, locomotor, has basal hook	Rudimentary, uniramous
Labrum	Large, without spine		Moderately large, no spine	Very large, no spine	Small at this stage, but large a little later
Mandible	Biramous, unjointed, no blade	Same	Same	Same	Rudimentary, uniramous
Metastoma	Rudimentary	Absent	Rudimentary	Five rudimentary body segments	A long unsegmented body without telson
First maxilla	Rudimentary	Absent	Rudimentary		
Second maxilla	Rudimentary	Absent	Rudimentary, with spine		
First maxilliped	Rudimentary	Absent	Rudimentary, bilobed		
Second maxilliped	Absent	Absent	Absent		
Third maxilliped	Absent	Absent	Absent		
First pereiopod	Absent	Absent	Absent		
Second pereiopod	Absent	Absent	Absent		
Third pereiopod	Absent	Absent	Absent		
Fourth pereiopod	Absent	Absent	Absent		
Fifth pereiopod	Absent	Absent	Absent		
First abdominal	Absent	Absent	Absent		
Second abdominal	Absent	Absent	Absent		
Third abdominal	Absent	Absent	Absent		
Fourth abdominal	Absent	Absent	Absent		
Fifth abdominal	Absent	Absent	Absent		
Sixth abdominal	Absent	Absent	Absent		
Telson	Absent	Present, with spines	Well marked, deeply notched, has spines		
Anus	Absent		Absent		

TABLE III.—Comparative table of the parts of the last *Nauplius* or meta-*Nauplius* of *Lucifer*, *Penæus*, *Euphausia*, and *Apus*.

	Second free *Nauplius* of *Lucifer*, $\frac{8}{1000}$th inch long (Plate 2, fig. 26).	Meta-*Nauplius* of *Penæus* (MÜLLER, Zeit. Zool., xix., fig. 2).	Meta-*Nauplius* of *Euphausia* (METSCHNICKOFF, Zeit. Zool.. xx., taf. xxxiv., fig. 5).	Second free *Nauplius* of *Apus* (BALFOUR, after CLAUS).
Compound eyes	Absent	Same	Same	Same
Ocellus	Present	Same	Same	Same
Frontal organ	Not observed	Same	Present	Same
Carapace	Indicated by fold	Well marked	Same, margins toothed	Elongated backwards
First antenna	Uniramous, unjointed, locomotor, long, sensory hairs	Same	Same	Same, but shorter than second
Second antenna	Biramous, unjointed, locomotor, no blade	Biramous, locomotor	Biramous, locomotor, jointed, five blades	Biramous, locomotor, obscurely jointed, has blade
Labrum	Large, no spine	Moderately large	Same	Very large
Metastoma	Rudimentary, double ?	?	Rudimentary	Not mentioned
First maxilla	Rudimentary	Same	Same	Same
Second maxilla	Rudimentary	Same	Same	A number of rudimentary body segments
First maxilliped	Rudimentary	Same	Same	
Second maxilliped	Absent ?	Rudimentary ?	Absent	
Third maxilliped	Absent	Absent	Absent	
Thoracic sornites	Absent	Absent	Absent	
Abdominal sornites	Absent	Absent	Absent	
Telson	Well marked, not forked, has spine	Deeply forked, with spines	Notched, has spines	Forked, has spines
Anus	Present	Present	Present	Present

TABLE IV.—Comparative table of the parts of the first *Protozoëa* in *Lucifer*, *Acetes* (?), *Sergestes*, *Penæus*, and *Euphausia*.

	1. First *Protozoëa* of *Lucifer*, $\frac{30}{1000}$th inch long (Plate 2, fig. 27).	2. CLAUS's larva (' Untersuchungen,' &c., plate iv., fig. 2).	3. *Protozoëa* of *Sergestes* (CLAUS, 'Untersuchungen,' &c., plate v., fig. 1).	4. *Protozoëa* of *Penæus* (MÜLLER, 'Verwandlung,' &c., plate ii., fig. 4).	5. *Protozoëa* of *Euphausia* (CLAUS, 'Untersuchungen,' &c., plate i., fig. 2).
Compound eye	Absent	Rudimentary	Stalked	Rudimentary	Absent
Ocellus	Present	Same	Same	Same	Same
Frontal organ				Present	Same
Shell gland	Present after first moult				Present
Carapace	½ length of body, rostrum, posterior, dorsal, and lateral spines	¾ length of body, postero-lateral spines	More than ½ length of body, rostrum, postero-lateral and dorsal spines	½ length of body, no rostrum or spines, edges smooth	More than ½ length of body, no rostrum or spines, edges serrated
First antenna	As long as second, jointed, uniramous, locomotor	Not quite as long as second, many joints	Seven joints	Same as *Lucifer*	Same
Second antenna	Biramous, jointed, locomotor, no blade	Same	Same	Same	Same
Labrum	Large, small spine	Small, no spine described	Has spine	Same	Same
Metastoma		Bilobed	Bilobed		Bilobed
Mandible	Blade, no palp	Same	Same	Same	Blade and rudiment of limb
First maxilla	Adult form, scaphognathite	Like *Lucifer*	Same	Same	Same
Second maxilla	Has scaphognathite	Same	Same	Same	Same
First maxilliped	Large, biramous	Same	Same	Same	Same
Second maxilliped	Rudimentary, biramous	Same	Large, biramous	Rudimentary, biramous.	Somite, no appendage
Third maxilliped	Somite, no appendage	A segmented region	Rudimentary	Segment, no appendage	Same
First pereiopod	Somite, no appendage	A segmented region	Somite, no appendage	Same	Same
Second pereiopod	Somite, no appendage	A segmented region	Somite, no appendage	Same	Same
Third pereiopod	Somite, no appendage	A segmented region	Somite, no appendage	Same	Same
Fourth pereiopod	Somite, no appendage	A segmented region	Somite, no appendage	Same	Same
Fifth pereiopod	No somite	A segmented region	Somite, no appendage	Same	Same
Abdominal somites	Represented by long unsegmented region about ⅓ the length of the body	Same, less than ¼ length of body	Same, less than ⅓ length of body	Same, ⅓ length of body, six abdominal somites appear, according to CLAUS, before next moult	Same, less than ⅓ length of body
Telson	Notched, four pairs large spines, one pair small ones	Deeply forked, six pairs of spines	Still more forked, with six pairs of spines	Deeply forked, seven pairs of spines	Posterior edge straight, seven pairs of spines

TABLE V.—Comparative table of the parts of the last *Protozoëa* of *Lucifer*, *Acetes*, *Sergestes*, *Penæus*, and *Euphausia*.

	Last *Protozoëa* of *Lucifer*, $\frac{18}{1000}$th inch long (Plate 5, fig. 48).	**Last *Protozoëa* of *Acetes*, $\frac{31}{1000}$th inch long (fig. 77).**	***Elaphocaris* of *Sergestes* (CLAUS, 'Untersuchungen,' taf. vi., fig. 1).**	**Last *Protozoëa* of *Penæus* (MÜLLER, 'Verwandlung,' &c., fig. 7).**	**Last *Protozoëa* of *Euphausia* (CLAUS, 'Untersuchungen,' taf. i., fig. 4).**
Compound eye	Rudimentary	Stalked, short.	Stalked, long	Stalked, short	Rudimentary
Ocellus	Present	Same	Same	Same	Same
Carapace	Dorsal spine, two postero-lateral spines, and rostrum, all simple	Same	Same, all compound	No posterior spines	No rostrum, dorsal spine only
First antenna	*Nauplius*-like, two jointed	Same, three jointed	Same, seven jointed	Like *Lucifer*, but with sense hairs	Like *Lucifer*
Second antenna	*Nauplius*-like	Same	Same, but without basal rings, no endopodite	Like *Lucifer*	Same
Labrum	Has spine	Same	No spine	Has spine	No spine
Mandible	Adult form, no palp	Same	Same	Same	Same
First maxilla	Two basal joints, jointed endopodite, scaphognathite	Same	Same	Same	Same, without scaphognathite
Second maxilla	Has scaphognathite	Same	Same	Same	Same
First maxilliped	Biramous, large	Same	Same	Same	Rami very short and simple
Second maxilliped	Small, biramous	Same	Large	Large	Somite, without appendages
Third maxilliped	Bilobed bud	Same	Small, but functional	Bilobed bud	Somite, without appendages
First pereiopod	Somite, rudimentary appendages	Same	Same	Same	Somite, without appendages
Second pereiopod	Somite, rudimentary appendages	Same	Same	Same	Somite, without appendages
Third pereiopod	Somite, rudimentary appendages	Same	Same	Same	Somite, without appendages
Fourth pereiopod	Somite, rudimentary appendages	Same	Same	Same	Somite, without appendages
Fifth pereiopod	Somite and appendages absent	Same	Somite, rudimentary appendages	Same	Somite, without appendages
First abdominal somite	Somite, no appendages	Same	Same	Same	Same
Second abdominal somite	Somite, no appendages	Same	Same	Same	Same
Third abdominal somite	Somite, no appendages	Same	Same	Same	Same
Fourth abdominal somite	Somite, no appendages	Same	Same	Same	Same
Fifth abdominal somite	Somite, no appendages	Same	Same	Same	Same
Sixth abdominal somite	Somite united to telson, appendages present as bilobed buds	Same	Same	Same	Same
Telson	Notched	Forked	Deeply forked	Same	Straight

TABLE VI.—Table of the parts of the Schizopod larva of *Lucifer*, *Acetes*, *Sergestes*, and *Penæus* at the first stage.

	Schizopod stage of *Lucifer* (Plate 6, fig. 50).	*Acanthosoma* stage of *Acetes* (fig. 84).	*Acanthosoma* of *Sergestes* (CLAUS, 'Untersuchungen,' taf. v., fig. 6).	Schizopod stage of *Penæus* (MÜLLER, 'Verwandlung,' &c., taf. 11, fig. 7).
Ocellus	Present	Same		
Compound eye	Stalk short	Same	Stalk, very long	Stalk short
Carapace	⅓ length of body, rostrum, two antero-lateral spines, no posterior spines	Same	⅓ length of body, rostrum, six pairs of antero-lateral spines, no posterior spines	⅓ length of body, rostrum, two pairs of antero-lateral spines, no posterior spines
First antenna	Plumose fringing hairs, long shaft, one short rudimentary flagellum	Same, with a second rudimentary flagellum	Same	Same
Second antenna	Rudimentary at first, but it acquires scale and flagellum at next moult	Scale and flagellum	Same, flagellum long	Scale large, flagellum rudimentary
Labrum	Has spine			No spine
Mandible	No palp		Same	Same
First maxilla	Like that of *Protozoëa*		Same	Same
Second maxilla	Scaphognathite rudimentary		Same	Same
First maxilliped	Rami equal		Endopodite longest	Same
Second maxilliped	Short exopodite, long endopodite, both free and movable	Exopodite alone free and movable	Equal, both free	Biramous
Third maxilliped	Short exopodite, long endopodite, both free and movable	Exopodite alone free and movable	Endopodite longest	Biramous
First pereiopod	Short exopodite, long endopodite, both free and movable	Exopodite alone free and movable and chelate	Exopodite very long, no chela	Endopodite rudimentary, exopodite very long, jointed
Second pereiopod	Short exopodite, long endopodite, both free and movable	Exopodite alone free and movable and chelate	Exopodite very long, no chela	Endopodite rudimentary, exopodite very long, jointed
Third pereiopod	Short exopodite, long endopodite, both free and movable	Exopodite alone free and movable and chelate	Exopodite very long, no chela	Endopodite rudimentary, exopodite very long, jointed
Fourth pereiopod	Short exopodite, long endopodite, both free and movable	Endopodite absent	Exopodite very long, no chela	Endopodite rudimentary, exopodite very long, jointed
Fifth pereiopod	Somite absent	Same	Exopodite very long, no chela	Endopodite rudimentary, exopodite very long, jointed
First abdominal	Somite present, appendages absent	Appendages rudimentary	Like *Lucifer*	Same
Second abdominal	Somite present, appendages absent	Appendages rudimentary	Like *Lucifer*	Same
Third abdominal	Somite present, appendages absent	Appendages rudimentary	Like *Lucifer*	Same
Fourth abdominal	Somite present, appendages absent	Same	Same	Same
Fifth abdominal	Somite present, appendages absent	Same	Same	Same
Sixth abdominal	Like adult	Same	Same	Same
Telson	Long, narrow, notched	Short, wide, notched	Short, deeply cleft	Very long, forked

VII. Serial Homology and Bilateral Symmetry in the Crustacea.

The Phyllopods and the highest Brachyura are connected with each other by a tolerably complete series of intermediate forms, and as we pass this series in review we cannot fail to notice that, as has been so frequently pointed out by morphologists, each successively higher form is a little in advance of the one next below it in the degree to which the functions and structure of the somites and appendages are subordinated to the individuality of the organism as a whole.

In the lower forms the body is made up of a series of nearly similar somites, and the appendages, with the exception of those at the anterior end of the body, are essentially alike in structure and their functions are indentical throughout the series. The greater part of the body of such a Crustacean as *Artemia* consists of a series of similar somites, and in *Apus* we find more than sixty pairs of limbs which agree with each other so perfectly in function as well as in structure that any one of them might be substituted for any other without involving any essential change in the structure of the animal as a whole.

At the other end of the series we have Crabs with the primitive distinctness of the somites so obscured by the centralised individuality of the whole organism that it cannot be traced at all without careful study and comparison of various stages in the life of a number of forms.

Comparing the various appendages of a Crab with each other we find that their functions are not at all alike. The mandibles are nothing but masticating organs, and the power which they once had, and which they still retain in the *Nauplius* of *Lucifer* to aid in locomotion, has entirely disappeared.

Other appendages have become organs for procuring food, or weapons of offence or defence; others have become walking legs; others long oars or paddles; others again have lost all limb-like functions, and are changed into accessory reproductive organs; whilst others again have entirely disappeared.

In accordance with this specialisation of each appendage to a particular function, a corresponding structural change has been brought about, and it is only after careful study of the younger stages that we perceive the mandibles, maxillæ, foot-jaws, walking and swimming legs, and copulatory organs of an adult Crab to be as strictly homologous with each other as are the unspecialised appendages of *Apus*.

The integration of the somites into a centralised whole has been accompanied by a differentiation of each appendage from the others, and a specialisation to a restricted function.

An adult Crab resembles and differs from one of the higher Macroura in about the same way that it resembles and differs from its own *Megalops* larva, and the transition from the larval form to the adult form is accompanied, like the transition from an adult low Crustacean to a high one, by increased dependence of the various parts on each other, by the increased prominence of the general individuality over the individualities of the somites or metameres, and by the increased structural and functional specialisation and differentiation of each appendage as compared with the others.

This series of changes is so well exemplified by the study of adult and larval Crustacea; it is so remarkable and interesting; so very conspicuous and unquestionable, that it has long attracted the attention and called forth the speculation of morphologists. It is natural to suppose that the process of change which is open to our observation through study and comparison of living Crustacea, is a continuation of a similar process which went on in the remote past. There seems then at first sight to be reason for believing that, if we could go far enough back, we should find the individuality of the whole organism gradually disappearing and giving place to the separate individualities of the component somites; that we should find the specialisation of the appendages gradually disappearing, until we should at last find, as the remote ancestor of the Crustacea, a series or community of independent organisms, each one essentially like the others, and able to provide for its own wants and to lead an independent existence when accidentally or naturally detached.

This view has been advocated at length by **Hæckel** ('Generelle Morphologie,' 1866) and by **Spencer** ('Principles of Biology,' vol. xi., 1867), and used by both these writers as an explanation of the origin of all segmented or compound animals and plants. It has been accepted, with more or less qualification, by many other writers, although **Huxley** ('Oceanic Hydrozoa') and **Metschnickoff** (Zeit. f. Wiss. Zool., xxiv.) have pointed out that, even in the Siphonophoræ, where the individualities of the units in the compound are extremely well marked, the view that the organism has been evolved by the gradual integration and specialisation of originally independent Zooids is attended with serious difficulties.

So far as we can see there is no reason why the Crustacea might not have originated in this way, by the gradual integration and differentiation of a community of independent metameres, but the evidence which is attainable seems to directly oppose the belief that this has actually happened. We are able to trace the higher Decapods back, very satisfactorily, to a Phyllopod-like ancestor with a long series of undifferentiated somites and appendages, but even here the somites are simply parts of the body, and they furnish no more evidence than those of a Crab to show that they ever were the independent organisms of a community.

When we attempt to go still further back we find that the facts of embryology, if they show any thing whatever about the phyllogeny of the Crustacea, lead us back to a *Nauplius* with three inter-dependent somites and three pairs of specialised appendages, rather than to a form with a great number of unspecialised somites and similar appendages.

Turning now to a somewhat different aspect of the subject, we notice that, if we confine ourselves to structure, and leave out of sight the question of origin, there is the closest similarity between serial homology and the homology between the corresponding organs of allied animals which OWEN has proposed to distinguish by the term "special homology."

The structural relation between one appendage of *Lucifer*, say the first pereiopod, and another, such as the swimmeret, is identical with the relation between the pereiopod of *Lucifer* and that of *Squilla*, or a Crab or Lobster. In both cases we have a fundamental similarity of plan, which is independent of external conditions; and joined to this essential similarity, we have a more superficial diversity of structure which is plainly due to difference in the functions of the appendages, and their relations to the external world. The resemblance between the two kinds of homology does not stop here. Tracing the ontogeny of the appendages we find that there is much less difference between the larval pereiopods of *Lucifer* and those of the Lobster than there is between the appendages of the adults, and we find exactly the same thing when we compare the pereiopod and swimmeret of the same individual at earlier and earlier stages of development.

There is precisely the same resemblance between symmetry and special homology. The right and left claws of the Common Crab (*Callinectes*) are not exactly alike, since the cutting edge of one claw is sharp and set with pointed teeth, while the edge of the other is thick, with thick blunt crushing tubercles. The two appendages are alike in plan or homologous, but each is fitted for a specialised function by a slight structural peculiarity. In this case, as in the others, the differences are less marked, and the common plan more closely followed, in the larva than in the adult.

Serial homology and bilateral symmetry are thus seen to be like special homology in all purely structural features. In each case the homology is a resemblance which is independent of external conditions, but which may be obscured by secondary modifications whenever external conditions render it necessary.

In each case, too, the secondary modifications become less marked, and the underlying plan more evident as we pass back from the adult to earlier and earlier stages of development. We must therefore include all three kinds of homology in a single class or category, and the employment of one term to denote the phenomena of special homology, of another for serial homologies, and a third for bilateral homologies, and others for other sorts of general homology must not be allowed to obscure the fact that they are all different forms of the same thing, essential similarity joined to superficial diversity. The terminology which has been employed by BRONN, HÆCKEL, LANKESTER, and others for the different kinds of homology is valuable, and the only reason why I have not made use of it is that the more familiar terms, "serial homology" and "bi-lateral symmetry" answer every purpose equally well in treating of the Arthropods. HÆCKEL'S subdivisions are natural, but they are simply subdivisions of a great class of similar phenomena, which must still be included under the general term "homology."

Special homology may be defined in two ways, morphologically and phylogenetically. From the morphological point of view an homology is a similarity in essential plan of structure, which may be obscured by differences due to diversity of function. From the phylogenetic point of view it is a resemblance which is due to community of origin or heredity from a common ancestor, while the differences between homologous organs are due to the divergence of allied forms, and to the selection and perpetuation, through natural selection, of variations which are in accordance with changed conditions of life.

Now are the phenomena of serial and lateral homology like those of special homology in this second or phylogenetic sense, as well as in a morphological sense ?

On the assumption that the remote ancestor of the Crustacea was a community of independent organisms, all of which had inherited their organisation from the same parent, we might answer that serial homology is like special homology when viewed from a phylogenetic stand-point, and if we assume that this series was at first double, and that the progress of centralisation suppressed one side of each metamere as the community became gradually fused into a bilateral organism, we may make the same statement regarding symmetry.

A process of evolution of this sort is not impossible, and in some cases there seems to be evidence that it has actually occurred. Pyrosoma is clearly a community of independent Ascidians, which has been brought by natural selection into a form which has a certain degree of individuality of its own, independent of that of the component units; although in this case the peculiar form of the community has called for little differentiation, and the polymorphism is therefore very slight.

The salpa-chain is a bilateral community, and in Doliolum we have a similar community which exhibits considerable polymorphism. If this process were carried a little further we might ultimately have a bilaterally symmetrical organism in which corresponding parts in the series or on opposite sides should be strictly homologous by descent; but we are not therefore justified in assuming that all instances of serial and lateral homology have originated in this way, and even if we were a more careful analysis will show that the assumption does not remove all the difficulties.

If we grant, for the sake of argument, that the Crustacea are not the descendants of a *Nauplius*, but of a remote ancestor which consisted of a community of independent metameres, we shall still be forced to recognise a bond of relationship between the limbs of a Decapod, which is very much more recent than that which they owe to common descent from the parent of the group of Zooids which formed the ancestral community.

A reference to the figures will show that the first, second, and third thoracic limbs of the adult *Lucifer* agree with each other, or are homologous, in certain features which are not present in a Schizopod. The exopodite is absent and the endopodite is long and slender in all of them, and it carries short hairs along its entire length, while, in the Schizopod-larva, the exopodite is present, and the long hairs are restricted to the tip of the stout endopodite. We must therefore recognise a bond of union or homology between these three appendages which has determined that they shall be like each other in the adult *Lucifer*, and the assumption that this similarity is due to heredity from the parent of the imaginary metameres which joined together to form the primitive Crustacean is out of the question, for we know that no further back than the Schizopods these appendages had quite a different structure.

The study of serial or lateral homology in other groups of animals forces us to the same conclusion, and compels us to recognise a persistent bond of union between them which cannot be due to what we usually understand by heredity.

On the assumption that the Vertebrates are the descendants of a community of metameres, the genetic relationship between a man's arm and a bird's wing must be almost infinitely closer than that between a man's arm and his leg, and this again much more recent than that between his right and his left arm. The arm and wing inherit their homology from the anterior limb of the common ancestor of man and the birds, but man's arm and leg have no common ancestor more recent than the limb of the parent of the imaginary metameres which gave origin, by their union, to the ancestor of the Vertebrates, and the common ancestor of the right and left arms must have been still more remote.

When we compare man's arm and leg we find that they have homologous features which are not only more recent than the time when man's ancestors diverged from the ancestors of the birds, but more recent than the separation of the anthropoid and simian stems. They resemble each other in the texture of the skin and in the shape of the nails, and these resemblances are strictly homological, that is, they are not due to external conditions, but in spite of them; and we meet with countless similar resemblances all through the animal kingdom. They are not accounted for by the "metamere" theory, even if this is fully accepted, for in many cases they are not old, but are of recent acquisition.

In the case of the Crustacea the assumption that the remote ancestor of the group had a many-jointed body does not account for them; and as the supposed necessity for an explanation of serial homology is the only reason for believing that this remote ancestor had a great number of body-segments, it is clearly illogical to reject the embryological evidence that this ancestor was a three-jointed *Nauplius*, in order to hold an hypothesis which fails to account for the facts which are supposed to render it necessary.

Explanation of the Plates.

All the figures where the magnifying power is not stated were drawn with a power of 160 diameters (Zeiss, Oc. 1, Obj. D); but the actual amplification of the drawings is not uniform. In copying the original sketches it has been convenient to reduce the size of some of them, and no inference as to relative size should be drawn from any of them except where measurements are given.

In order to render the figures as truthful and lifelike as possible, the animals were subjected to very little confinement while under examination, and as their incessant and violent movements rendered the use of a camera impossible, they are not drawn to a fixed scale.

In all the Plates the capital reference letters are used to denote the same parts, as follows:—

A. First antenna.
A. 1 to *A*. 6. The series of abdominal somites.
An. Second antenna.
C. Carapace.
E. Compound eye.
L. Labrum.
M. Mandible.
Mp. 1, *Mp*. 2, *Mp*. 3. The first, second, and third maxillipeds.
Mx. 1, *Mx*. 2. The first and second maxillæ.
Oc. Ocellus.
Pl. 1–*Pl*. 6. The six pairs of abdominal appendages.
Pr. 1–*Pr*. 4. The four pairs of thoracic limbs.
R. Rostrum.
T. 1–*T*. 4. The four thoracic somites.
T. Telson. (*Tl*. in Plate 3, fig. 26.)

PLATE 1.

The letter *a* in figs. 1 to 8, which were all drawn from the same egg, marks the same point in all.

Fig. 1. An egg during the period of rest which follows the first period of segmenting activity.

Fig. 2. The same egg at the beginning of the second period of segmenting activity, five minutes after the stage shown in fig. 1.

Fig. 3. The same egg five minutes later. One of the primary segments is beginning to divide into two.

Fig. 4. The same egg ten minutes later. One of the primary spherules is perfectly divided into two, and the division of the other is less advanced.

Fig. 5. The same egg five minutes later, and completely divided into four equal segments. This stage ends the second period of segmenting activity.

Fig. 6. The same egg, fifteen minutes later, during the second period of rest.

Fig. 7. The same egg, ten minutes later, entering upon the third period of activity.

Fig. 8. The same egg, twenty-five minutes later, divided into eight equal spherules. This stage ends the third period of activity.

Fig. 9. Another egg during the third period of rest.

Fig. 10. Another egg near the end of the fourth period of activity, and divided into sixteen equal spherules, arranged around a central segmentation cavity.

Fig. 11. Optical section along the principal axis of a somewhat older egg, showing the yolk spherule, *c*, and the segmentation cavity, *b*.

Fig. 12. Optical section of the same egg at right angles to the one shown in fig. 11.
b. The segmentation cavity.
c. The yolk spherule.

PLATE 2.

Fig. 13. An optical section of an egg somewhat older than the one shown in fig. 11.
Fig. 14. Optical section at right angles to that of fig. 13.
Fig. 15. Optical section of an egg a little older than the one shown in fig. 13.
d. Orifice of invagination.
Fig. 16. Optical section of a still older egg in the same position as figure 15.
Fig. 17. Optical section of a still older egg in the same position.
Fig. 18. Surface view of the formative pole of the egg shown in fig. 17.
Fig. 19. Optical section along the principal axis of a still older egg.
Fig. 20. A similar section of a still older egg.
Fig. 21. Ventral view of an embryo in the egg-shell 24 hours after oviposition.
e. Anterior end of body.
f. Large spherules in the region of the digestive tract.
g. Metastoma.
Fig. 22. Dorsal view of the same embryo.
(*e* and *f* as in fig. 21.)
h. Cerebral ganglia.
i. Pigment spots.
m. Muscles.
Fig. 23. Similar aspect of the same embryo artificially removed from the egg shell. (Letters as in fig. 22.)

PLATE 3.

Fig. 24. Ventral view of the same embryo, seen from a point of view a little anterior to that of fig. 21.
ex. Exopodite.
en. Endopodite.
(The other letters as in fig. 21.)
Fig. 25. Side view of the *Nauplius*, $\frac{8}{1000}$th inch long, as it leaves the egg 36 hours after oviposition.
g. Metastoma.
p. Pigment spots.
ex. Exopodite.
en. Endopodite.

Fig. 26. Side view of a *Nauplius*, $\frac{9}{1000}$th inch long, and a little older than the one shown in fig. 25. The animal is a little flattened by pressure. In a ventral view of the same larva the anus was visible at the point marked *a.* in the figure.

œ.	Œsophagus.
s.	Stomach.
l.	Large cells around stomach.
i.	Intestine.
ga.	Cerebral ganglia.
n.	Ventral nerve-chain.
p.	Pigment spots.

Fig. 27. Dorsal view of a *Protozoëa*, $\frac{20}{1000}$th inch long, which moulted from the *Nauplius* shown in fig. 26, about 96 hours after oviposition.

ga.	Cerebral ganglia.
s.	Stomach.
h.	Heart.
i.	Intestine.
p.	Pigment spots.
a.	Anus.
m.	Muscles of œsophagus.
ls.	Postero-lateral spines of carapace.
ds.	Posterior dorsal spine of carapace.
abd.	Unsegmented portion of abdomen.
ex.	Exopodite.
en.	Endopodite.

Fig. 28. Right mandible of the same specimen seen from below.

Fig. 29. Right mandible seen from behind.

Fig. 30. Back or outer surface of first maxilla of the same specimen.

sc.	Scaphognathite.

Fig. 31. Posterior surface of left-second maxilla of the same specimen.

sc.	Scaphognathite.
b.	Basal joint.
en.	Endopodite.

Fig. 32. Left-first maxilliped of the same specimen; posterior surface.

ex.	Exopodite.
en.	Endopodite.
b.	Basal joint.

Fig. 33. Right-second maxilliped of the same specimen.

ex.	Exopodite.
en.	Endopodite.
b.	Basal joint.

- Fig. 34. A *Protozoëa*, $\frac{27}{1000}$th inch long, and a little older than the one shown in fig. 27.
    - *œ.* Œsophagus.
    - *pp.* Anul pigment spots.
    - (Other letters as in fig. 27.)

PLATE 4.

- Fig. 35. Side view of another larva at the stage shown in fig. 34.
    - *ga.* Cerebral ganglia.
    - *s.* Stomach.
    - *sg.* Shell gland.
    - *h.* Heart.
- Fig. 35A. Surface view of the area of attachment of the œsophageal muscles of the same larva.
- Fig. 36. Second antenna of the same specimen.
    - *b.* Basal joint.
    - *ex.* Exopodite.
    - *en.* Endopodite.
- Fig. 37. Mandible, at same stage, seen from below.
- Fig. 38. First maxilla of left side at same stage.
    - 1 and 2. The two cutting joints of the basal portion.
    - *ex.* Exopodite.
    - *en.* Endopodite.
- Fig. 39. Left-second maxilla at same stage.
    - *b.* Basal portion.
    - *en.* Endopodite.
    - *sc.* Scaphognathite.
- Fig. 40. Left-first maxilliped at same stage.
    - *ex.* Exopodite.
    - *en.* Endopodite.
- Fig. 41. Left-second maxilliped at same stage.
    - *ex.* Exopodite.
    - *en.* Endopodite.
- Fig. 42. Ventral view of the same larva after the next moult and $\frac{35}{1000}$th inch long.
- Fig. 42A. Outline of the anterior end of fig. 42, more enlarged.
    - *b.* Basal joints of appendages.
    - *ex.* Exopodites.
    - *en.* Endopodites.
    - *sc.* Scaphognathite.

PLATE 5.

Fig. 43. Ventral view of the last *Protozoëa* (Dana's *Erichthina demissa*), $\frac{50}{1000}$th inch long.
  ex. Exopodite of second antenna.
  en. Endopodite of second antenna.

Fig. 44. Dorsal view of the same larva (Zeiss, A. 2).
  ds. Median dorsal spine of carapace.
  ls. Postero-lateral spines of carapace.

Fig. 45. Side view of the same larva at the same stage.
  ds. Median dorsal spine of carapace.
  tg. Thoracic ganglia.
  i. Intestine.

Fig. 46. Posterior surface of left first maxilla of the same larva.
  1 and 2. Cutting joints of basal portion.
  en. Endopodite.
  sc. Scaphognathite.

Fig. 47. Left second maxilla of the same larva.
  b. Basal joint.
  sc. Scaphognathite.
  en. Endopodite.

Fig 48. Left first maxilliped of same larva.
  b. Basal joint.
  ex Exopodite.
  en. Endopodite.

Fig. 49. Left second maxilliped of same larva.
  (Letters as in fig. 48.)

PLATE 6.

Fig. 50. First Schizopod stage (this stage is the equivalent of Dana's genus *Sceletina;* of Claus's *Acanthosoma* stage; and of Willemöes-Suhm's *Amphion* stage). The larva passes, by a single moult, which was frequently observed in isolated specimens, from the stage shown in fig. 43 to the one which is shown in this figure.

Fig. 51. Mandible at same stage.

Fig. 52. First maxilla at same stage.

Fig. 53. Second maxilla at same stage.

Fig. 54. *Sceletina* larva, two moults later (Zeiss, A. 2).
  s. Antero-lateral spine.
  p. Pigment spot on fourth abdominal somite.

Fig. 54A. Outline of anterior end of carapace.
Fig. 55. Dorsal view of the anterior end of the same larva.
Fig. 56. First antenna of the same larva.
Fig. 57. Second antenna of the same larva.
Fig. 58. Mandible at same stage.
Fig. 59. Locomotor appendages of the left side, at same stage, seen from above.

PLATE 7.

Fig. 60. Side view of a young *Lucifer*, about $\frac{25}{100}$th inch long, which was produced by the moulting of a larva like that shown in fig. 54 (this stage agrees pretty exactly with the *Mastigopus* stage of *Sergestes*).
- *g.* Antennal gland.
- *n.* "Neck."
- *c.* Carapace.

Fig. 61. Half-grown *Lucifer*, about $\frac{1}{2}$ an inch long.
- *g.* Antennal gland.
- *cg.* Cerebral ganglia.
- *co.* Commissures to ventral ganglia.
- *s.* Cephalic pouch of stomach.
- *h.* Heart.
- *c.* Carapace.

Fig. 62. Inner surface of mandible of adult.
Fig. 63. Outer surface of mandible of adult.
Fig. 64. Outer surface of first maxilla of adult.
Fig. 65. Inner surface of same.

PLATE 8.

Fig. 66. Head of a small female about $\frac{2}{3}$rds of an inch long, seen from below.
Fig. 67. Second maxilla of same.
Fig. 68. First maxilliped of right side of adult.
Fig. 69. Inner surface of same.
Fig. 70. Second maxilliped of adult.

PLATE 9.

Fig. 71. Dorsal view of tip of telson of adult female.
Fig. 72. Side view of last abdominal somite and swimmerets of adult female.
Fig. 73. The same parts of an adult male.

Fig. 74. Side view of posterior half of carapace and first abdominal somite of a mature female, to show the reproductive organs. In order to reduce the number of figures this specimen is represented with its ovaries full of ripe eggs, while a large bunch of developing eggs are attached to the basal joints of the last pair of thoracic appendages, but these two features are never exhibited completely at the same time in a single individual.

ca.	Carapace.
h.	Heart.
i.	Intestine.
tg.	Thoracic ganglia.
ov.	Ovary.
od.	Oviduct.
sr.	Seminal receptacle.

Fig. 75. The corresponding part of the body of an adult male.

c.	Carapace.
h.	Heart.
i.	Intestine.
tg.	Thoracic ganglia.
a.	First abdominal somite.
t.	Testis.
vd.	First division of vas deferens.
sp.	Second division.
sv.	Third division.

Fig. 76. First pleopod of young male.
Fig. 78. Side view of the larva shown in Plate 10, fig. 77 (Zeiss, A. 2).
Fig. 79. Dorsal view of the same larva.
Fig. 80. Mandible of the same larva.
Fig. 81. First maxilla of the same larva.

en.	Endopodite.
sc.	Scaphognathite.

Fig. 82. Second maxilla of the same larva.

sc.	Scaphognathite.
en.	Endopodite.

PLATE 10.

Fig. 77. Ventral view of the last *Protozoëa* stage of *Acetes* $\frac{67}{1000}$th inch long.

ex.	Exopodite of second antenna.
en.	Endopodite of second antenna.

Fig. 85. The specimen shown in fig. 84, after another moult and $\frac{80}{1000}$th inch long.
Fig. 86. First antenna of the same larva.
Fig. 87. Second antenna of the same larva.

PLATE 11.

Fig. 84. Ventral view of the specimen shown in fig. 81, after the next moult and $\frac{70}{1000}$th inch long.

ex. Exopodite.

en. Endopodite.

Fig. 83. First maxilliped of the larva shown in Plate 10, fig. 77.

Fig. 88. Second and third maxillipeds and periopods of the larva shown in Plate 10, fig. 85.

Fig. 89. First pleopod of same larva.

Fig. 90. An older specimen $\frac{15}{100}$th inch long, which was captured at the surface.

III. *On the Structure and Development of the Skull in Sturgeons* (Acipenser ruthenus *and* A. sturio).*

By William Kitchen Parker, *F.R.S.*

Received April 14,—Read May 5, 1881.

[Plates 12–18.]

INTRODUCTION.

Several years ago the late Mr. William Lloyd procured for me from Hamburgh seventeen young Sturgeons (*Acipenser sturio*); these were from 7 to 8 inches in length. Valuable as these specimens were, they were far too much developed for embryological purposes; and no pains were spared by me to obtain, if possible, newly-hatched embryos and small "fry" of this type.

More lately it was suggested to me by Mr. Balfour that I should write to Professor W. Salensky, of Kasan, who had been working at the development from the egg of *Acipenser ruthenus*, the small Sturgeon or "Sterlet."† My application to him was promptly and most kindly responded to, and in a short time I received a considerable number of newly-hatched and very small young of that species, ranging from 5½ to 14½ millims. in length. Half these were for Mr. Balfour, and the rest for me. His researches are embodied in that inestimable work, the second volume of his 'Comparative Embryology;' mine are here offered to the Society in the accustomed form.

I purposely refrain at present from working out the structure and development of the trunk and limbs (I have laboured at these regions, and shall be ready to resume that part of my work when this is done); but other workers are from time to time taking up those parts, and when once the cephalic skeleton is mastered what remains will be a comparatively easy task.

I am more anxious to be prepared for my own limited work by acquiring a thoroughly clear conception of the embryology of each type; in this my best helper is Mr. F. M. Balfour. Professor Huxley is, and always has been, my most valued critic and counsellor in all that relates to broad and philosophical views of animal life generally, and of the life of the Vertebrata in particular.

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* The skull described in my last paper as that of *Discoglossus pictus* (Phil. Trans., 1881, Plate 20, figs. 7–11, p. 112) was prepared from a badly-preserved, half-grown *Rana esculenta*. I am indebted for this correction, and for a genuine *Discoglossus*, to M. Boulenger.

† This species rarely exceeds the length of 3 feet (Günther, 'Study of Fishes,' p. 361).

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2. "On the Ganoid Fishes of the British Carboniferous Formations." Trans. Palæont. Soc., 1877 (Part I.: Palæoniscidæ), pp. 1–60; plates 1–7.
3. "On the Structure and Affinities of the Platysomidæ." Trans. Roy. Soc. Edinb., 1879, vol. xxix., pp. 343–391; plates 3–6.

WIEDERSHEIM, Dr. ROBERT. 'Morphologische Studien.' Part III.: Das Skelet und Nervensystem von *Lepidosiren annectens* (*Protopterus*, ang.) Plates 2 and 3. Jena: 1880.

First Stage.—Newly-hatched embryos of Acipenser ruthenus *from 5½ millims. to 6½ millims. in length.*

The specimens figured by me (Plate 12, figs. 1–3) are rather smaller than the one figured by Mr. BALFOUR ('Comp. Embryol.,' vol. ii., p. 88, fig. 53) which measured 7 millims. in total length, but his description may serve for mine, which were nearly as much developed as his slightly larger specimen.

After describing the development of the embryo, Mr. BALFOUR explains its peculiar outspread form in its unhatched condition (*op. cit.*, p. 86, fig. 52), and says:—"The further changes which take place are, in the main, similar to those in other Ichthyopsida, but in some ways the appearance of the embryo is, as may be gathered from fig. 52, rather strange. This is mainly due to the fact that the embryo does not become folded off from the yolk in the manner usual in the Vertebrates; and as could be shown in the sequel, the relation of the yolk to the embryo is unlike that in any other known Vertebrate. The appearance of the embryo is thus something like that of an ordinary embryo slit open along the ventral side and then flattened out. Organs which properly belong to the ventral side appear in the lateral parts of the dorsal surface. Owing to the great forward extension of the yolk the heart (fig. 52, B) appears to be placed directly in front of the head." Then, after describing the progress made in the development of the brain and organs of special sense, Mr. BALFOUR says:—"At the sides of the cephalic plate the visceral arches make their appearance, and in fig. 52, A and B, there are shown the mandibular (*Md.*), hyoid (*Ha.*), and first branchial (*Br.*) arches, with the hyo-mandibular (spiracle) and hyo-branchial clefts between them. They constitute peculiar concentric circles round the cephalic plate, their shape being due to the flattened form of the embryo already alluded to." And then, further on (p. 88) our author says:—"Before hatching, the embryo has to a small extent become folded off from the yolk both anteriorly and posteriorly; and has also become to some extent vertically compressed. As a result of these changes, the general form of its body becomes much more like that of an ordinary Teleostean embryo. The general features of the larva after hatching are illustrated by figs. 53, 54, and 55. Fig. 53 represents a larva of about 7 millims., and 54 a lateral and 55 a ventral view of a larva about 11 millims.* There are only a few points which call for special attention in the general form of the body. In the youngest larva figured" [fig. 53, 7 millims. long; and see also Plate 12, figs. 1–3 of the present paper of somewhat younger larvæ] "the ventral part of the hyomandibular cleft is already closed: the dorsal part of the cleft is destined to form the spiracle (*sp.*). The arch behind is the hyoid, on its posterior border is a membranous outgrowth, which will develop into the opercular membrane. In the older larvæ a very rudimentary gill appears to be developed on the front walls of the spiracular cleft, but I have not succeeded in satisfying myself about its presence; and rows of gill papillæ have appeared on the hyoid, and the true branchial arches (figs. 54 and 55, *g*)." [The mandibular gill, about which Mr. BALFOUR speaks doubtfully, is according to my observations a thickened mass of hypoblastic cells lining the front wall of the first or "spiracular" cleft. This mass is crescentic (Plate 12, figs. 2, 4, and 7, cl^1.), and is slightly grooved along its hinder margin; on each side of this groove the low ridges are imperfectly divided into little rounded masses, which appear to me to be, evidently, the rudiments of branchial papillæ; at the lowest computation, this soft, tuberculate mass of cells is homologous with the tracts of cells which do develop into branchial papillæ in the Elasmobranchii.] "The biserially-arranged gill papillæ of the true branchial arches are of considerable length" [see Plate 12, figs. 4–9], "and are not yet covered by the operculum, but they do not form elongated thread-like external gills like those of the Elasmobranchii. The oral cavity is placed on the ventral side of the head; it has a more or less rhomboidal form. It soon however (fig. 55) becomes narrowed to a slit with projecting lips, which eventually becomes converted into the suctorial mouth of the adult. The most remarkable feature connected with the mouth is the development of provisional teeth (fig. 55) on both jaws. These teeth were first discovered by KNOCK ('Die Beschr. d. Reise z. Wolga Behufs. d. Sterlettbefructung,' Bull. Soc. Nat., Moscow, 1871). They do not appear to be calcified, and might be supposed to be of the same nature as the horny teeth of the Lamprey. They are, however, developed like the teeth, as a deposit between a papilla of subepidermic tissue and an epidermic cap. The substance of which they are formed corresponds morphologically to the enamel of ordinary teeth. As they grow they pierce the epidermis and form hollow spine-like structures with a central axis filled with subepidermic mesoblastic cells. They disappear after the third month of larval life. In front of the mouth two pairs of papillæ grow out, which appear to be of the same nature as the papillæ in the suctorial disc of Lepidosteus (figs. 54, 55, p. 89). They are very short in the embryo, represented in fig. 53. Soon, however, they grow in length (figs. 54 and 55, *st.*)" [Plate 12, figs. 4, 5, 7, 9], "and it is" [*more than*] "probable that they become the barbels, since both sets of structures occupy a precisely similar position."

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* These do not correspond precisely to the specimens worked out by me, and of which I also have given figures illustrating the outward form. My first Stage includes larvæ from 5½ millims. to 6½ millims. in total length; Stage two, 8½ millims. to 9½ millims.; and Stage three, 13½ millims. to 14½ millims. The largest larva of this species in those sent to me was 14½ millims. ($\frac{7}{12}$ths of an inch) in total length,

"The openings of the nasal pits are at first single, but the opening of each becomes gradually divided into two by the growth of a flap on the outer side (fig. 54, *ol.*)" [Plate 12, figs. 4, 7, 9, *ol.*]. "It is probable that the two openings of each nasal sack, so established in these and in other Fishes, correspond to the external and internal nares of the higher Vertebrata. At the time of hatching there is a continuous dorso-ventral fin" [Plate 12, figs. 1, 2], "which by atrophy in some parts, and hypertrophy in other parts, gives rise to all the impaired fins of the adult, except the first dorsal and the abdominal. The caudal part of the fin is at first symmetrical, and the heterocereal tail is produced by the special growth of the ventral part of the fin combined with an atrophy of the dorsal part."*

Referring the reader to Mr. BALFOUR's work and to my own plates, I shall now simply describe the figures of the general form of the first stage, and then those of the second and third, in which also will be given a detailed account of their skeletal structures.

The fourth, fifth and sixth stages will be of the other species (*A. sturio*), and that in a thoroughly metamorphosed condition.

The smallest of my specimens (5½ millims. long, Plate 12, fig. 1) is intermediate between figs. 52 and 53 of Mr. BALFOUR (*op. cit.*, pp. 87, 88); the original of his fig. 52 (A and B), may be found in Professor SALENSKY's Russian work (plate 7, figs. 56 and 58), whilst his fig. 53 is only slightly larger than my second specimen (Plate 12, figs. 2 and 3).

In the smallest specimen, scarcely hatched at the time when it was put into the spirits, the large hind-brain (C^3.) and the lesser mid- and fore-brain (C^2., C^1.) are seen arranged in an accurate series. The auditory sac (*au.*) is below and behind the wide, front part of the hind-brain, the eye (*e.*) below and between the fore- and hind-brain, and the olfactory sac (*ol.*) on the side of and below the fore-brain, in front of the eye. The mouth (*m.*) is a considerable rhomboidal space on the ventral aspect of the head, and behind it we see three folds, two clefts, and a general hollow space behind the third fold. These folds are the rudiments of the mandible, hyoid, and first branchial (*mn.*, *hy.*, br^1.), the clefts are the hyomandibular (cl^1.) and the hyo-branchial (cl^2.), and the fossa behind leads to the tissues that are preparing to form the rest of the branchial arches with their intervening clefts. The last third of the spinal region is behind the anus (*an.*); it is the tail, and the intestinal opening is but a small distance behind the huge enlargement caused by the yolk-mass; that mass is three-fifths the length of the embryo. A vertical line drawn through the hind-brain and eye-ball would bisect a special enlargement in front of the yolk-mass. This enlargement, caused by the heart (*h.*), would be partly in front of that line, and looking at the embryo from the upper end of the line we should see the heart lying *in front of the head.*

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* I am glad to be able to give, and the reader will not be sorry to have, an account of these early larvæ in Mr. BALFOUR's own words. Of course I have been over the same ground, but my work has been made much easier by my friend's kindness in sending me his proofs as soon as they were in type.

The liver (*l.*) would be behind the lower end of the line, for most of the yolk-mass which is included in the rudimentary stomach lies *in front of the liver*, contrary to what takes place in the Lamprey, Frog, Elasmobranch, and the Amniota (BALFOUR, *op. cit.*, p. 90, fig. 56). According to that excellent observer "the peculiar flattening out of the embryo on the yolk is due to the mode in which the yolk becomes enveloped by the hypoblast" (p. 91).

In an embryo more developed, but slightly smaller (Plate 12, figs. 2, 3, 6½ millims. long) than that figured by Mr. BALFOUR (his fig. 53 was 7 millims. long), the yolk-mass is only one-third the length of the embryo—in the last it was half as long—and the tail is now also about one-third the length of the whole larva. The head is now fairly free from the yolk-mass, at least as far as to the hyoid fold (*hy.*), but the heart (*h.*) is a very short distance behind the mouth (*m.*), and the end of the intestine (*an.*) is seen to be three times as far from the yolk-mass (*y.*). The tail is at present perfectly diphycercal; the notochord running along the middle of the upturned tail, and the azygous fin is equally above and below the axis.

The mouth (fig. 3, *m.*) is a crescentic slit, with a small anterior bay in the middle of its anterior convex margin, and with a definite labial rim. In front of the oral opening and under the fore-brain (C^1.), behind a transverse ridge that passes from side to side parallel with the mouth, there are two pairs of pupiform enlargements with their narrow ends behind, and those of each pair approximated; these are the rudiments of the barbels (*bb.*). There are four visceral folds with their clefts, the two anterior larger folds being the mandibular and the hyoid; from the latter a free edge is growing, the opercular fold (*op.*); between the two bars the first (or hyomandibular) cleft (cl^1.) is closed ventrally, already, its permanent uppermost part will be the spiracle. The convex front lip of that limited cleft is thickening inside, and tends to produce a mandibular gill. The nasal sac (*ol.*) is still a shallow pouch with a thickened rim, the outer coats of the eye (*e.*) are closing over the lens, and the auditory sac (*au.*) is a smallish oval mass showing no evident involution.

In this larger embryo the basis-cranii and visceral arches are still composed of "embryonic cartilage;" these skeletal structures can be better studied in somewhat more advanced larvæ.

Second Stage.—Larva of Acipenser ruthenus, *from 8½ to 9½ millims. in length.*

The larvæ at this stage have a very *Selachian* appearance (Plate 12, figs. 4–8); altogether they have made a very great advance in development since the last stage: I shall describe first the external form, then a dissection of the visceral arches, and after that a series of transverse sections.

The azygous fins are beginning to be subdivided (fig. 7); now, the axis (notochord) turns downwards behind, but the tail is still diphycercal; it has become almost as long as the trunk, measuring from the anal aperture (*an.*), so that the post-anal region is very much longer, relatively, than in the last stage (fig. 2). The azygous fin is more definite along the trunk, almost reaching as a crest to the head, and the pectoral fins (*p.f.*) that then were at most mere thickenings of the tissue, are now well-developed, free, auriform paddles.

The yolk-mass (*st.*) makes only half the bulging it did: it is now quite portable; the intestine (*in.*, *an.*) is now seen as a definite ventral cavity behind the stomach and yolk, and is nearly as long as the wide part in front.

The heart (figs. 5 and 7, *h.*) is now fairly behind the operculum (*op.*), but is still seen from the outside as a double swelling.

The mesocephalic flexure is less, and the various parts of the brain (C^1., C^2., C^3.) are very visible through the diaphanous membrano-cranium.* So also the auditory organs show clearly through the skin, and the common tube of the anterior and posterior canals (*au.*, *p.s.c.*), and the single lesser tube forming the horizontal canal (*h.s.c.*), are very apparent without dissection.

The eye-ball (*e.*) is rapidly finishing and its socket is being formed, and there is a distinct suborbital ledge. The olfactory sac (*ol.*) is lateral and close to the eye, it has acquired a very definite superficial membranous capsule; this is sending downwards a triangular flap which tends to subdivide the opening.

The "barbels" (*bb.*) are now well formed, and look in the larger specimens of this stage like formidable *tusks*; they arise from a recess in front of the upper lip, which has over it a fold of the fore face, and in front of it, below the fore brain (Plate 12, fig. 5), there is between the olfactory sacs (*ol.*) a very remarkable concavity; it is nearly circular, is as large as one of the olfactory sacs, is almost as deep, and has in front of it a crescentic fold, and on each side a lateral lip-like margin. Its somewhat produced hind margin lies between the two inner barbels, and its lateral walls run up to those barbels. In the lesser larvæ of this stage (figs. 4, 5, *m.*) the mouth is still somewhat rhomboidal, but in the larger (fig. 7) its edges come very near together. The lips are now thick and large, the front is emarginate, the hind lip has an apiculate lobe, at the mid-line, looking forward; each is armed with the small, clear, pointed teeth (*t.*).

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* Through want of proper food these larvæ soon began to starve. Moreover, the spirit in which they were preserved contracted their tissues; the figures *purposely* show this contracted appearance.

There are, now, seven cartilaginous arches on each side, the two first are large, the rest smaller; they are all out of sight, more or less, the operculum hiding the two first, and the gills those behind; the clefts also are largely hidden from view by these structures.

In the smaller specimens at this stage (fig. 4) the form of the two first arches can be seen through the skin, the pterygo-quadrate bar running within the upper lip, MECKEL'S cartilage within the lower jaw, and the hyoid arch (*hy.*) inside the fore part of the opercular fold (*op.*). The opercular fold of the mandible is very limited on account of the closing in of the ventral part of the first cleft (cl^1.), but its serial homology with the large next fold (*op.*) is manifest. Inside it is thickened by a mass of cells from the first hypoblastic pouch, and this mass, somewhat obscurely, shows on its surface two rows of short mammillate projections—arrested gill papillæ.

But the next fold, with its lining, is greatly developed (Plate 12, figs. 4–7, *op.*), and its inner face is covered with rather long papillæ, like those which grow freely from the proper branchial arches (*br.*). Membranous, at present, this free fold of the hyoid arch is destined to contain three ganoid scutes, the upper of which (the opercular) will be one of the largest of the huge plates developed in this fish.

The rich growth of simple longish papillæ on the other arches hides them from view; these gills, however, are much shorter than those seen in the large embryos of Selachians.

When the skin and the gills are removed from the side of the head in one of the larger larvæ at this stage—9½ millims. in length—then the form of the newly chondrified bars is seen (Plate 13, fig. 6).

I have already remarked upon the *Selachian* appearance of the larvæ at this stage (Plate 12, fig. 7); a dissection, like the one under notice, displays much more than superficial Selachian characters. Even in the adult the Sturgeon has not made much advance towards the Teleostean culmination of the fish-type, but the dermal scutes have been largely dominated, in the head, by the cartilaginous endo-skeleton; and in the face, behind the mandible, we find many of the bars ensheathed in their own "ectostoses."

The hyostylic type of skull, which is not seen in the generalised forms, such as *Notidanus* and *Cestracion*, but is common to Skates and ordinary Sharks, is in the "Acipenseridæ" carried to its uttermost pitch of perfection, and here, in this minute larva, recently hatched, and scarcely chondrified, the Selachian type of skull is overpassed, and *at least two* additional segments are seen on each side. These additional joints in the highly subdivided and modified primary hyoid bar are expressly for the purpose of throwing the mouth out far away from the skull; the jaws are suspended on an extremely hypertrophied *upper hyoid segment.*

This hypertrophy of the "epi-hyal" segment is correlated with the atrophy of the uppermost, or suspensorial part of the pier of the mandible, which pier, even in this early larva, is *below the middle* of the hyoid arch Plate (13, fig. 6, *pg.q.*, *mn.*, *hm.*, *c.hy.*).

This is the more remarkable in a type that might have been expected to show something primitive in its visceral arches, seeing that it is only a *Chondrosteus Ganoid*, with a huge, permanently undivided notochord, and a totally unossified chondrocranium.

Even in the Salmon (Phil. Trans., 1873, Plates 1–3), the embryos of which are much larger and more tractable, I did find, *as cartilage*, the primitive facial bars; and their metamorphosis into the highly modified hyostylic type could be traced step by step; but this cannot be done in *Acipenser* nor in *Lepidosteus* (my next subject). In *Scyllium* (Zool. Trans., vol. x., plate 34) this was also possible; but in the Skates, *Raia* and *Pristiurus* (ibid., plates 35 and 39) the earliest tracts of true hyaline cartilage were, already, half metamorphosed, for all the *uppermost* segments (ibid., plate 35, fig. 4) were developed as distinct cartilages, although the remainder of each arch was continuous, and its segmentation could be traced afterwards.*

Here, in *Acipenser*, whilst the cartilage is still very soft, the segments of the highly subdivided hyoid arch are all apparent, although the branchial arches are continuous tracts (Plate 13, fig. 6); but their "pharyngo-branchial" segment—so very distinct in the embryo of the Skate—is already half severed from the main bar (Plate 13, fig. 4, *p.br.*, *e.br.*).

The pier of the mandibular arch, instead of growing up to the basis cranii, is a falcate cartilage (the pterygo-quadrate, *pg.q.*), which lies half-way down the face, and passes forwards and downwards. It is flat, obliquely-placed, and somewhat uncinate in front; behind, it is thick and rounded, and forms a condyle which lies in the concavity of its own lower segment, the mandible (*mn.*). This segment is thick behind; in front of its condyloid depression it sends upwards a coronoid swelling, whilst, behind, it ends in a rounded angular process. The rest of the bar is rounded; it lessens towards its ventral end, and is curved the opposite way to the pterygo-quadrate.

The rounded quadrate region of the upper bar is attached by ligamentous fibres to the lower end of the hyoid pier; that pier is the hyomandibular (*hm.*), the lower, rounded end of which is being segmented off as the symplectic (*sy.*). The whole bar is a massive phalangiform cartilage, gently bent backwards, altogether running downwards and a little forwards, and articulated above by an oblong condyle, to the "tegmen tympani" of the ear-capsule, under the horizontal canal (*h.s.c.*). It is one-third larger than the pterygo-quadrate, but its own lower piece, the cerato-hyal, is somewhat less than the mandibular pier.

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* In Selachians themselves, the best of all Fishes in which to study the development of these parts, it is next to impossible to maintain a consistent nomenclature of the elements of the visceral arches. In the Dog-fish (*Scyllium*, ibid., plates 37, 38) the hyomandibular is evidently the serial homologue of the epibranchials; in the Skate (*Pristiurus*, ibid., plate 35) the uppermost part of all the arches is developed as a separate nucleus of cartilage, and the metapterygoid and hyomandibular naturally classify themselves with the succeeding pharyngo-branchials. Again, the subdivided hyoid pier of *Acipenser*, which also carries the mandibular apparatus, is not divided into a normal pharyngo- and epi-hyal, but the epi-hyal, apparently, is itself subdivided, and there is no pharyngo-hyal.

The cerato-hyal element is a flattish rod, pinched in the middle, and dilated at both ends; its upper end is articulated to the postero-inferior face of the rudimentary symplectic by means of a small intercalary nucleus of cartilage—the inter-hyal (*i.hy.*). The lower end of the bar is nearly segmented off as a short semi-oval hypo-hyal (*h.hy.*). The branchial arches (*br.*) are about half the size of the hyoid—supposing it to have been in one large bar; there are *five* of them, and they decrease in size from before, backwards. The top of each is semi-distinct from the rest as a pharyngo-branchial (*p.br.*); that part is turned inwards, the rest of the arch is curved outwards, and turns inwards and forwards, below, where it joins the basi-branchial bar (Plate 13, fig. 4, *b.br.*).

Second Stage (continued).—*Transversely-vertical and vertical sections of the head of a larva of* Acipenser ruthenus, 9½ *millims. in length.*

The transverse sections (Plate 12, figs. 1–3, and Plate 13, figs. 1–5) will further illustrate the structure of the visceral arches, and also show the state of the basis-cranii at this stage.

Section 1.—The first section (Plate 12, fig. 10) is through the fore-brain (C^1.), and the fore part of the mid-brain (C^2.), the eye-balls (*e.*), and the barbels (*bb.*); here the only cartilaginous tracts are the trabeculæ (*tr.*), which are sub-oval in section, obliquely placed, and separated by a distance equal to their own width; they are sub-concave on their upper surface, where they lie under the thalamencephalon as it narrows to the infundibulum.

Section 2.—The rounded ends of the trabeculæ are free for a very short distance in the extremely short head of this larva, which has at present no rudiment of the huge pre-cranial rostrum afterwards to be developed. In this section (Plate 12, fig. 11) the trabeculæ are wider, flatter, and are in close contact with each other; they lie here under the infundibulum (C^1.) which is covered now directly by the mid-brain (C^2.). Here the fore part of the oral cavity is cut through, so that the front row of teeth (*t.*) are seen, and on each side the outer barbels (*bb.*) are just in view; this section is behind the eye-ball. Above the mouth two oval masses of cartilage, more solid than the trabeculæ, are seen; these are the pterygo-quadrate bars (*pg.q.*) at their fore end.

Section 3.—The next (Plate 12, fig. 12) is close in front of the auditory capsules, through the widest part of the mid-brain (C^2.) where it joins the hind-brain, in the middle of the mouth, and the huge Gasserian ganglia (V.), whose cerebral roots are brought into view. Here the trabeculæ (*tr.*) are in close contact, are wider than in the last section, and they curve well round the base of the hind part of the infundibulum (*inf.*).

Here the pterygo-quadrate cartilages (*pg.q.*) are very solid and curved, and have a sharp edge lying over the "adductor mandibulæ" muscle (*ad.m.*), (see also SALENSKY, *op. cit.*, plate 18, fig. 165, *ad.m.*); these muscles lie outside the concave face of these oblique cartilages; the front teeth and lip are still seen. Above the front face of the auditory capsules (here, by mistake, lettered *e*), the auditory involutions (*aq.v.*) are seen.

Section 4.—The next slice (Plate 13, fig. 1) contains the hinder half of the auditory involution (*aq.v.*), and below it the auditory capsule (*au.*) is seen, in the region of the anterior canal; there is some cartilage in the infero-lateral part of the wall of the capsule. This is behind the mid-brain, and the hind-brain (see figs. 1–3, C^3.) is very large; large masses of ganglionic cells are seen belonging to the 5th, 7th, and 8th nerves (V., VII., VIII.). Outside, the lip of the spiracle (cl^1.) is cut through, and below the brain the large notochord (*nc.*) is shown near its apex. This lies in and between the moieties of the investing mass (*iv.*), which are nearly square—just a little hollow above, and convex below. Below these parts the oral cavity is laid open; right and left of it the thinner hind part of the pterygo-quadrate (*pg.q.*) is severed, and, below, the teeth of the lower lip (*t.*, *l.l.*).

Section 5.—Here the hinder part of the ganglionic mass belonging to the 7th and 8th nerves (Plate 13, fig. 2, VII., VIII.), and some fibres of their cerebral roots are cut through; the spiracle ($cl^{1'}$.) is here laid open; above it a cartilaginous tract of the auditory wall is seen. The anterior and horizontal canals, and the vestibule (*a.s.c.*, *h.s.c.*, *vb.*) are laid open. The notochord (*nc.*) is enlarging, and is being enclosed in the square moieties of the investing mass (*iv.*) which lie somewhat under it. Below the mouth the mandibles (*mk.*) are laid bare throughout their entire length, on account of their almost directly transverse position, and over their articular hollow the articular knob (*q.*) of the pterygo-quadrate cartilage is shown *in situ*. The back of the lower lip (*l.l.*) is seen, whose toothed front face was shown in the last section.

Section 6.—Here, on each side of the narrowing hind-brain (Plate 13, fig. 3, C^3.), the auditory capsules are cut through in their widest part, and both the arch and ampulla of the posterior canal (*p.s.c.*) are cut and laid open, as well as the deep saccular part of the vestibule (*vb.*). The infero-lateral shell of auditory cartilage is joining itself on to the more thin and outspread part of the investing mass (*iv.*—line too high); the notochord (*nc.*) is placed directly above the moieties of cartilage. The pharyngeal cavity is here very large, and in its walls we see the large, massive, arcuate epi-hyal element, undergoing segmentation into a long hyo-mandibular and a short symplectic piece (*hm.*, *sy.*). Here the directly transverse position of the parts gives, in one section, the whole of the hyoid series, for the symplectic is seen to be followed, lower down, by the little inter-hyal (*i.hy.*), and that by the long cerato-hyal (*c.hy.*), which is giving off from its sub-triangular end the short hypo-hyal (*h.hy.*). This is the front face of the section, and behind and within the hyoid we see the lower half of the first branchial arch and the fore part of the basi-branchial bar ($c.br^1$.); the branchial arch, like the hyoid, is becoming subdivided below, to form the hypo-branchial. The front part of the ganglionic mass of cells for the 9th and 10th nerves (IX., X.) is cut across.

Section 7.—These most successful and illustrative sections* display all that is sought for; here (Plate 13, fig. 4), on each side of the narrowing hind-brain (C^3.), the notochord has become suddenly twice as wide as in the last, and it has now gained a lower position; the investing mass (*iv.*) is horn-shaped in section, and the base of each "horn" sits on the supero-lateral face of the huge notochord. The back wall of the auditory capsule (*au.*) is cartilaginous, and at this part is some distance from the basal plate (*iv.*); in that interspace the ganglion of the vagus (X.) is shown.

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* The sections were made by my son Mr. W. N. Parker.

Here, happily, another visceral arch, the second branchial (*p.br.*, $e.br^2.$, $c.br^2.$, *b.br.*) is exposed in one slice; the segments are only indicated by the crowding of cells at the line where segmentation will take place. Within and behind the second branchial arch the third ($c.br^3.$) is partly seen, and the large, simple gills (*g.*) are shown fringing the folds of the section, below; they are clavate, because of the loop of the simple capillary vessel within.

Section 8.—Behind the ear-capsules the notochord has a diameter only two-fifths less than that of the hind-brain (Plate 13, fig. 5, *nc.*, $C^3.$); the investing mass (*iv.*) embraces it more below, and is rising, right and left, so as to form the lower half of the occipital arch (*e.o.*). Behind the gills, under the narrowed pharynx, the yolk-mass is now seen.

A vertical section of the head in one of the larger larvæ of this stage (9½ millims. long; Plate 14, fig. 1) shows the low position of all the parts of the fore-brain ($C^1.$, $C^{1a}.$, *inf.*), the forward position of the mid-brain ($C^2.$) and the huge size of the hind-brain ($C^3.$). The cavity of the fold of the mid-brain runs equally forwards and upwards, and the infundibulum (*inf.*) looks backwards and is quite distinct from the pituitary rudiment (*py*). The notochord (*nc.*) nearly reaches the latter, it is very large, arcuate, and bent downwards in front, showing no disposition to enter the fold of the mid-brain in the "post-clinoid" region. Two of the barbels (*bb.*) are seen to the right of the mid-line, and behind these we see the thick upper lip (*u.l.*) bounding the mouth (*m.*), which opens directly below the pituitary rudiment. It is a rather narrow passage, and has in its hinder boundary the thick lower lip (*l.l.*); in this the mandible (*mk.*) is seen at its ventral end, and the fore end of the pterygo-quadrate cartilage (*pg.q.*) is seen in the upper lip; these cartilages lie a little to the right of the section, which is exactly in the middle. In the pharynx, above and behind the oral chink, the first three clefts ($cl^{1-3}.$) come into view, below these part of the yolk (*y.*), and in front of the yolk the heart (*h.*).

Third Stage.—Larvæ of Acipenser ruthenus *from 13½ millims. to 14½ millims. in total length.*

Amongst the more advanced larvæ of this Fish only one reached the length of 14½ millims., or $\frac{7}{12}$ of an inch; the next in size were 1 millim. shorter. One of these latter was made into horizontal sections, and others were *dissected;* the unique specimen was made into vertically-transverse sections.

The external appearance of these, next in size to the last, viz., 13½ millims., is extremely Selachian (Plate 12, fig. 9, and Plate 13, figs. 8, 9). The tail is more than *dosteus*, and in many types, they are really continuous under the pituitary body, as in *Scyllium* and *Raia*, forming a complete floor to the skull for some distance in front of the notochord and the post-clinoid wall; but this part has a small fontanelle immediately below the pituitary body (see fig. 1, *py.*). Opposite the optic nerves they part for a short distance, nearly meet again, and then diverge as two very short out-turned horns, the tips of which are the rudiments of the cornua trabeculæ (*c.tr.*). Between the Gasserian ganglion and the optic nerve (V., *e.*) the trabeculæ grow upwards, forming the rudiment of the alisphenoid (*al.s.*); the walls and roof, however, will be best studied in the sections.

In front of the eye-balls (*e.*), between them and the olfactory sacs (*ol.*), there is a definite antorbital cartilage growing outwards and a little forwards from the trabeculæ; this is the ethmo-palatine (*e.pa.*), a familiar element, which, here, is only semi-segmented from the trabecula. In Skates, as in Urodeles and Teleostei, it is distinct; but in Batrachia and Sharks it is not; it is well seen as a "process" in *Notidanus* (*Heptanchus* and *Hexanchus*, see GEGENBAUR'S 'Selachians,' plate 1, and also in embryos of *Scyllium*, T. Z. S., vol. 10, plate 37, fig. 1, *a.o.*). Here, it helps to form the back wall of the crypt in which the olfactory sac (*ol.*) is lodged; in these Fishes, as in *Lepidosteus* and the Teleostei, there is no distinct "paraneural" cartilage for the nasal pouch.

Leaving the walls and roof of the skull, until I come to the sections, I shall now describe the visceral arches, which are greatly developed since the last stage.

In a side view of these parts (Plate 13, fig. 11) I have shown the arches relative to the other cephalic structures, which are left in outline to give prominence to the view of the arches themselves.

In another figure (Plate 13, fig. 12) the arches are shown as seen from above, and in a third (Plate 14, fig. 5) they are shown as seen from below. The hyoid arch is twice as large as the others, and has undergone most segmentation and specialization; the branchial arches decrease in size and in the amount of their segmentation, from before, backwards.

The proper pedicle of the mandibular arch, in this extremely *hyostylic* type, is not only absent, but the stunted and forwardly projecting "pier" is much further from the normal point of attachment—under the outgoing fifth nerve—than in any of the Selachians. Each of these pterygo-quadrate bars (*pg.q.*) is a curved plate of cartilage, thick and bent forwards in its articular region, where it grows out over the temporal muscle, somewhat, to form the rudiment of the "orbitar process," and flat as it widens out in its pterygoid region.

The right and left plates meet each other for some distance in front, a notch separating their round ends; they are gently convex, above, and gently concave, below. These bars reach as far forwards as the antorbital region, but are embedded in the very loosely attached palatal skin, and are thus only slightly connected with the basis cranii. In the angle formed by their convergence a pyriform wedge of somewhat newer cartilage (*mt.pg'.*) has made its appearance since the last stage; this is, as far as I know, an unique structure, and but for this stage I should have suspected that this keystone piece had been formed by the fusion of a right and left nucleus of cartilage. Others form round it afterwards, but this is the first, as it is also the most important, morphologically, of the pieces that form the *hind palate* of the Sturgeon.

It would not have been difficult to have dealt with the paired pieces that appear afterwards (Plate 16, figs. 5 and 6), as they are manifestly the counterparts of the single or multiple "metapterygoid" cartilages of the Rays. In *Torpedo* (GEGENBAUR'S 'Selachians,' plate 13, fig. 3, and plate 20, fig. 1, Kr., *a*, *b*,*) there are four of these cartilages on each side, but as a rule there is only one in that group, where it is a true metapterygoid segment of the pier, and not a mere "ray," as in the Shark. In Carinate Birds, where the skeleton reaches the utmost limits of specialization, remnants of Ichthyopsidan palatal cartilages reappear—ethmo-palatine, post-palatine, &c.; and in the Woodpeckers ("Picidæ") the palatine membrane bones are joined together under the basis cranii by an ossified cartilage, the medio-palatine (Trans. Linn. Soc., second series, "Zoology," vol. i., plates 1–5).*

MÜLLER simply calls the whole *compound* plate of the adult *Acipenser ruthenus* (*op. cit.*, plate 9, fig. 71, A, B) "unpaariger Gaumenknorpel," and the pterygo-quadrate bars "paariger Gaumenknorpel," so that the interpretation of these parts is left open. I shall merely, for the present, classify this plate with the free cartilages of the "Raiidæ," and call the median piece the "azygous metapterygoid" (*mt.pg'.*); the later pieces, right and left, will simply be called "paired metapterygoid segments" (*mt.pg''.*). In the adult (Plate 18, fig. 4) Mr. HOWES finds a row of four more much smaller segments along the mid line, in front of the main piece.†

The mandibles (*mn.*, *mk.*) are about as long as the upper bars, but after forming a shallow articular cavity on their thick upper end they become rounded rods, and their position is almost transverse, as these dissections, and the transverse sections (Plate 15, fig. 3, *mk.*) demonstrate; they do not quite meet in the middle, at present.

The hyoid arch has five segments in it on each side, but, like the mandible, no basal segment below. The uppermost piece, the hyomandibular (Plate 13, figs. 11, 12, *hm.*) is by far the largest element in the whole series of arches; above, it is articulated by an oval condyle to the under surface of the pterotic ridge, under the horizontal canal ("tegmen tympani"), and below by a cylindroidal condyle to the symplectic (*sy.*).

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* The median cartilage seen in the "Myxinoids" (MÜLLER, 'Myxinoids," plate 3, figs. 2–5, U), the "Gaumenplatte"—which partially unites with the trabeculæ in the adult *Bdellostoma* and *Myxine*, and *early* and *totally* in *Petromyzon*, is not part of the palate, as MÜLLER'S term would suggest, and as I once thought, but is, as Mr. BALFOUR showed me, the "intertrabecula"—*i.e.*, its hinder or interorbital part.

† In the scanty living remnants of the "Chondrosteous Ganoids," only the "Acipenseridæ" show this peculiar structure; the "Polyodontidæ" show no trace of it (see MÜLLER'S 'Myxinoids,' plate 5, fig. 7, there called *Planirostra edentula*; TRAQUAIR, 'Ganoid Fishes of the British Carboniferous Formations,' Part I., "Palæoniscidæ," Palæont. Soc., 1877, plate 7, fig. 1; and BRIDGE, "On *Polyodon folium*," Phil. Trans., 1878, Part II., Plates 55–57, pp. 683–733.

Its form is like that of a phalangeal segment, it is curved backwards, gently, and has an enlargement there, above the middle, the rudiment of the "opercular process." Its axis is coincident with that of the symplectic and the mandible (*sy.*, *mn.*, *mk.*), together they form a crescentic series, passing downwards and gently forwards, and having the convex margin behind; they are bent inwards below (fig. 12). The curve forward, as well as downward, brings the lower end of the symplectic under the pituitary body; this cartilage (*sy.*) is only one-fourth the length of the upper piece (*hm.*), but it is as thick as its thickest part; it is scooped above for the hyomandibular, and rounded below, where it is tied, by ligaments, to the two elements of the first arch (*pg.q.*, *mn.*, *mk.*).

In the Teleostei the inter-hyal is articulated to the inside of the non-segmented cartilage uniting the hyo-mandibular and the symplectic; in the Sturgeon it is articulated to the inside of the distinct symplectic, rather below the middle—at least at this stage (Plate 13, fig. 11, *i.hy.*). The inter-hyal is a small piece of cartilage, wedge-shaped, with its narrow end upwards; its broad lower end articulates with the top of the cerato-hyal, which is bilobate above, the inter-hyal articulating with the larger upper lobe (fig. 12, *i.hy.*, *c.hy.*).

The cerato-hyal is a thickish, somewhat sigmoid bar, it passes inwards and forwards and nearly meets its fellow of the opposite side; there is no basi-hyal, but a semi-oval segment is formed out of the ventral end of each cerato-hyal; this is the hypo-hyal (*h.hy.*).

The branchial arches (Plate 13, figs. 11, 12, and Plate 14, fig. 5) are larger than the lower part of the hyoid at first, but they lessen backwards, so that the last is only one-fourth as solid as the first. The first three develop a hypo-branchial segment (*h.br.*) exactly like the hypo-hyal, but larger. The inturned dorsal end of all but the last is segmented off as an epi-branchial (*e.br.*), and its apex becomes distinct as a small pharyngo-branchial (*p.br.*) in the first four arches.

The basi-branchial (*b.br.*) is a thickish rod, rounded in front and compressed behind; it only carries the first three arches; the fourth and fifth meet below.

Bony scutes are now developing in the skin, and in the skin of the mouth and the mucous membrane of the palate and fauces several bony plates have appeared; all these are determinable.

In the upper and lower views (Plate 13, fig. 12, and Plate 14, fig. 5) these plates, *most of which are dentigerous*, are shown in their relation to the visceral arches and the largest of these bones; the maxillary and dentary—of one side—are shown separately (Plate 14, fig. 6, *mx.*, *d.*). There is no determinable premaxillary, which like the nasals, and vomers, only exists *as one or many* of a great number of generalised scutes that are to be found in those respective regions. The maxillary (fig. 5, *mx.*) runs across and meets its fellow in front of the pterygo-quadrate (*pg.q.*); it is a long, subarcuate bar, with thickened edges, and carrying five or six sharp, recurved teeth on its anterior third; it is somewhat notched and bent in its hinder third. The dentary (*d.*) is almost exactly like it, but gently bent in the opposite direction; its teeth look a little forwards and fit in between those of the upper jaw.

The palatine (Plate 14, fig. 5, *pa.*) is a styloid tract of bone binding on the external edge of the broad fore part of the suspensorium (*pg.q.*); it is edentulous. The pterygoid (fig. 5, *pg.*) carries teeth; it is a thin plate of bone, which lies inside the hinder convex edge of the cartilage.

Over the rudimentary "orbitar process" of the suspensorium a small scale of bone is visible, just as when the squamosal is formed over the quadrate in the Amphibia;* this is the preopercular (*p.op.*). In the roof and sides of the pharynx inside the first branchial arch there are three pairs of small, arcuate, dentigerous scales of bone (Plate 13, fig. 12, and Plate 14, fig. 5); these are "upper pharyngeal bones." Clamping the hinder part of the hyomandibular, its rudimentary "opercular process," the opercular bone (*op.*) is seen; it is a convex shell, with spurs in front and a sharp round margin behind. These bones are evidently all mere parostoses; the "ectosteal" sheaths of the visceral arches have not appeared yet; the cranium never acquires any well-grafted bony plates.

Third Stage (continued).—*Horizontal sections of one of the lesser larvæ of* Acipenser ruthenus, *at this stage*—13½ *millims. long.*

Section 1.—The uppermost of these sections (Plate 13, fig. 10) is through the hemispheres and the base of the mid-brain directly over the thalamencephalon (C^{1a}., C^{1}.), the eye-balls (*e.*), and the front and sides of the mouth (*m.*). The quadrate and pterygoid regions (*q.*, *pg.q.*) are severed, being cut through where there is a depression above; moreover, the broad pterygoid plate is obliquely placed, and thus this horizontal section shows its thickness, but not its breadth. Between the hooked fore ends of these cartilages a newer tract, the lozenge-shaped azygous metapterygoid (figs. 10 and 11, *mt.pg'.*), is seen. Behind the transversely oval quadrate region the lower part of the hyomandibular (*hm.*) is cut across; it is obliquely placed, and between the two sections of cartilage, on the outside, the lower part of the first cleft or spiracle (cl^{1}.) is laid open. Part of the "adductor mandibulæ" muscle (*ad.m.*) is seen outside the pterygo-quadrate bar, and the opercular fold of the first cleft is seen with its rough inner surface.

Section 2.—The next section, below the last, is through the whole length of the pterygo-quadrate bar (Plate 14, fig. 2, *pg.q.*), which is seen to be of almost uniform thickness, and of a sigmoid shape; the fore end is turned inwards, and the hind part outwards. Behind it the top of the articular region of the transverse mandible (*mk.*) was sliced off. The basis cranii (*tr.*) is cut through close in front of the post-pituitary wall and the notochord, where the trabeculæ are completely confluent; the large infundibulum (*inf.*) is severed at its "neck" (see Plate 14, fig. 1, *inf.*). Here, this process of the brain is above and in front of the distinct pituitary body (fig. 1, *py.*), and seems to be distinct from the fore-brain (C^1.); this, however, is due to the manner in which it has been cut through on its projecting part. In this section the rudimentary hemispheres (C^{1a}.) are cut through where they pass forward from the first cerebral vesicle (figs. 1 and 2, C^1., C^{1a}.). The small olfactory sacs (*ol.*) are cut across; they lie outside the junction of the hemispheres with the thalamencephalon; the eye-balls (*e.*) are cut through in the lower part.

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* The squamosal of the Amphibia has a large descending *preopercular process*; but there is no separate preopercular bone, such as Fishes possess.

Section 3.—The lowest section (Plate 14, fig. 3) is through the lower part of the curved head, and takes in the whole boundary of the mouth (*m.*; see also fig. 1). The olfactory capsules are removed, and the barbels (*bb.*) are cut across at their root; each, at present, contains a pith of true cartilage. The fore part of the pterygo-palatine and the quadrate region of the same (*pg.q.*, *q.*) are shown as severed, and the latter is seen as a transverse process, bulging below to form the hinge of the jaw. Behind the mouth (*m.*) the lower lip is seen with its teeth (*l.l.*, *t.*), and in the substance of this visceral fold the whole length of the mandibles (*mk.*) is shown, first thick, in the articular region, then slenderer, and again enlarging distally.

Third Stage (continued).—*Transversly vertical sections of the head of the largest larva of* Acipenser ruthenus; 14½ *millims. long.*

Section 1.—The first section (Plate 14, fig. 7) is through the barbels (*bb.*) at right angles to the horizontal section just described (fig. 3), and also to the axis of the down-bent brain which is here developed into the rudimentary hemispheres (C^{1a}.). The plane of this section would form an angle of 45° with the plane of a section taken vertically through the hind part of the hind-brain, or through the front of the yolk mass and heart in such a larva as is shown in a longitudinally-vertical section (Plate 14, fig. 1, C^3., *y.*, *h.*). That, however, was a younger larva, and the head in this had straightened a little.

The hemispheres (fig. 7, C^{1a}.) are here shown to be, together, sub-oval in section, somewhat grooved above, and slightly protuberant below. The olfactory sacs (*ol.*) have no roof in front, and their floor is formed by the ethmo-palatine cartilage (*e.pa.*), which is distinct, in front, from the trabecula. The skin of the fore face, however, is developed into a valvular fold around the opening, and the mucous membrane is applied to the rising and falling of the structures that encompass it; there is no proper "paraneural" cartilage over the nasal capsule; in this, *Acipenser* agrees with *Lepidosteus* and the *Teleostei*, and differs from the Selachians, Cyclostomes, and Amphibia. Here the floor (*e.pa.*) is flat and distinct, and there is a considerable space between it and the trabecula (*tr.*) which is thick at its outer edge, and thins out towards its fellow of the opposite side, which it does not quite meet.

Section 2.—The next section (Plate 14, fig. 8) is through the widest part of the hemispheres (C^{1a}.) which are nearly oval in the section, from side to side, the top being convex as well as the bottom; this latter part is, however, narrower than the upper.

Here the roots of the front teeth are cut through in the base of the upper lip (*u.l.*, *t.*), and here the nasal pouch (*ol.*) has both roof and floor; the latter is continuous, now, with the trabecula (*tr.*) and bulges downwards, whilst the roof (*s.ob.*) is the fore end of a large tract of superorbital cartilage, which grows independently of the floor for some distance backwards—a very common thing in the Ichthyopsida.

This upper band is the lateral rudiment of the tegmen cranii, which in this type so soon covers in the great fontanelle and becomes so very massive; at present it is very similar to the palato-trabecular band (*e.pa.*, *tr.*) below; here the trabeculæ are wider apart than in the last section.

Section 3.—In this third pre-oral section (Plate 14, fig. 9) the projecting fore part of the mid-brain is seen to lie on the fore-brain (C^2., C^1.). This is through the back part of the olfactory pouch (*ol.*), which is becoming complicated; its palato-trabecular floor (*e.pa.*, *tr.*) is, here, at its widest and most solid part, and the superorbital roof over the nasal sac is narrower, and is sharp at its outer edge; the roots of the front teeth are still in view (*u.l.*, *t.*).

Section 4.—This is through the fore part of the eye-balls (Plate 14, fig. 10, *e.*); there is, here, a very solid lateral ethmoidal wall uniting the superorbital band and the trabecula (*s.ob.*, *tr.*); the latter is losing its palatine extension, outwards, and the two plates are still a good distance apart beneath the fore-brain (C^1.). Here the antorbital wall is cut through; it is membranous, at present.

Section 5.—In this section (Plate 14, fig. 11) the superorbital band (*s.ob.*) is simply a moiety of the tegmen cranii, for the projecting part is gone, and each band is creeping towards its fellow over the front of the mid-brain (C^2.). So also, below, the trabeculæ (*tr.*) have lost their palatine projection and they are creeping towards each other under the fore-brain (C^1.); these latter are the larger plates, and are rather indented below in the middle. Opposite the eye-ball, the front third of which is cut across, there is a small tract of cartilage cut through; it is rounded above and sharp below; this is a tract which runs backwards as far as to the Gasserian ganglion (see Plate 15, figs. 2, 3, *al.s.*, V.); it is orbito-sphenoidal here (*o.s.*), and alisphenoidal behind.

Section 6.—The lens of the eye (Plate 14, fig. 12, *e.*) is now reached, and the sclerotic is becoming cartilaginous; this section is very similar to the last, but the superorbital and orbito-sphenoidal bands (*s.ob.*, *o.s.*) are thinner, and the trabeculæ (*tr.*) are approximating; here the fold of the upper lip (*u.l.*) is cut through; it is seen to be folded off from the fore face, so that at this point it seems to be attached by a narrow isthmus; the next two figures will explain this (see also Plate 13, fig. 11).

Section 7.—This and the next (Plate 14, figs. 13, 14) are through the highest (or deepest) part of the brain, for here the bulging mid-brain (C^2.) lies right over the fore-brain (C^1.) in front of the infundibulum, which is cut through behind the second of these (Plate 14, fig. 1, *inf.*). Here the growing roof and walls of the orbital region of the skull are composed of *three* bands on each side (*s.ob.*, *o.s.*, *o.s'.*), for over the emerging optic nerve a lower tract of cartilage has appeared, which is thick on its inner face, growing into the sulcus between the swellings of the brain (C^2., C^1.). Here the large optic foramen is a "fenestra," as in Frogs; its upper margin is formed by this lower orbito-sphenoidal process (*o.s'.*), and its lower edge by the trabecula (*tr.*); this plate now touches its fellow of the opposite side. The upper lip (*u.l.*) is seen in all its depth here, and the teeth (*t.*) are in full view; they are looked at from behind.

Section 8.—In this slice (Plate 14, fig. 14) the hinder half of the eye-ball and optic nerve (*e.*, II.) is seen, and the brain (C^1., C^2.) is here at its greatest bulk. The trabeculæ (*tr.*) are confluent, are thicker, and rise more at the sides; this is close in front of the post-pituitary wall. The thick hind part of the orbito-sphenoid is now ready to become alisphenoidal; its two parts are molten together, and the upper tract or superorbital (*s.ob.*) is now thickening as it approaches the auditory sac, ready to become the postfrontal ("sphenotic") wing.

Close behind the upper lip (*u.l.*) two curved plates of cartilage are cut across; these are the pterygo-quadrate bars (*pg.q.*), they are thick outside, and bevelled where they meet.

Section 9.—In this section (Plate 15, fig. 1) the mid-brain (C^2.) is at its widest part and overlies the infundibular end of the fore-brain (*inf.*), to which the pituitary body (*py.*) is becoming attached; this is through the hinder part of the eye-balls (*e.*), and close in front of the Gasserian ganglion (see fig. 2, V.).

The superorbital band is now the "sphenotic" (*sp.o.*); below it there is a large membranous fenestra, and the short oval section of the narrow alisphenoidal band (*al.s.*). The trabeculæ (*tr.*) are apart again, where the pituitary body (*py.*) passes down; they are altogether narrower here, and are grooved by vessels below. Here the pterygoquadrates (*pg.q.*) are cut through their middle part, and here, in the re-entering angle behind their upper junction, a considerable wedge of cartilage is seen; this is the azygous metapterygoid (*mt.pg'.*); it has a pair of smaller pieces (*mt.pg''.*) or lateral metapterygoids attached to it; underneath this arched palate a number of teeth (pterygoid teeth) are seen.

Section 10.—This section (Plate 15, fig. 2) escapes both the eye-balls and ear-sacs, and is through the body of the Gasserian ganglion (V.) and behind the pituitary body. Here the trabeculæ are confluent again in front of the low post-pituitary wall and the end of the notochord. The sphenotic cartilage (*sp.o.*) is now ear-shaped in section, convex outside, and has two sub-concave inner faces, one applied to the skull-wall and one hanging down free. The pterygo-quadrates (*pg.q.*) are cut across a little in front of the hinge of the lower jaw, they are acuminate-oval in section, with the point downwards. The lower lip with its teeth (*l.l.*, *t.*) is here displayed, and the large "adductor mandibulæ" (*ad.m.*) is cut across its belly.

Section 11.—This (Plate 15, fig. 3) is the first of the auditory sections; the capsules (*au.*) are severed in their antero-superior-angle, to which is attached the upper and lower processes of the sphenotic (*sp.o.*). Through the wall of the capsules the ampulla of the anterior canal (*a.s.c.*) is seen. The alisphenoid (*al.s.*) re-appears in this section, and is twice as large as in fig. 1; the Gasserian ganglion (V.) is here cut through behind, and lies within its proper boundaries, namely, with the alisphenoid (*al.s.*) above and the investing mass (*iv.*) below. This section is near the edge of the low post-clinoid wall (Plate 14, fig. 4, *p.cl.*) and the apex of the notochord (*nc.*), which here lies on the gently-scooped plate formed by fusion of the two parachordal bands (*iv.*). The pterygo-quadrate (*pg.q.*, *q.*) is cut across close in front of the hinge, above and below, for it appears in two parts on account of its curve downwards to form the hinge. MECKEL's cartilages also (*mk.*) are cut through close to the hinge; they are placed across, behind the mouth, are gently arcuate, and slowly lessen towards the meeting point. Outside the hinge, an angle of the symplectic (*sy.*) has been cut off.

Section 12.—This section, half of which was drawn (Plate 15, fig. 4) like the last, is a *front view;* here the cavity of the auditory capsule is laid open, with the ampulla and part of the arch of the anterior semicircular canal (*a.s.c.*). The sphenotic cartilage is still seen above and below this front part of the capsule; from the capsule to the investing mass is the foramen ovale with the ganglion of the 5th (V.), perhaps also part of the "ganglion geniculatum," which belongs to the facial and auditory nerves (Plate 14, fig. 4. VII., VIII.); the notochord (*nc.*) still lies on the investing mass (*iv.*). The hinge of the lower jaw is seen from its front face; the rod of the mandible (*mk.*) was in the last section, and this shows the articular region. The quadrate end of the pterygo-quadrate (*q.c.*) is seen in its full size with its orbitar process (*or.p.*); outside the hinge the symplectic (*sy.*) is shown, and the hypo-hyal and half the cerato-hyal (*h.hy.*, *c.hy.*), and some of the teeth of the lower lip (*t.*), towards the mid line.

Section 13.—In this section (Plate 15, fig. 5) the hind-brain (C^3.) is becoming narrower, and each auditory capsule under the arch of the anterior canal (*a.s.c.*), is cut through so as to expose the ampulla of the horizontal canal (*h.s.c.*); above, there is a rudimentary tegmen cranii, continuous with the capsule and the hind part of the "sphenotic" tract. At this part the capsule, as is the rule in Fishes, is open towards the hind-brain, and in the fissure, below, the "ganglion geniculatum" (VII., VIII.) is shown. Here the capsules and the basal plates (*iv.*) are quite confluent, and the notochord (*nc.*) divides the latter, lying down between the two halves. The front face of this section was figured; here the massive symplectic (*sy.*) is shown in its front half, and a considerable portion of the cerato-hyal (*c.hy.*); the hypo-hyals are lost in this section, they were shown in the last (fig. 4); the basi-branchial, the first hypo-branchials, and part of the first cerato-branchials (*h.br*^{1}., *c.br*^{1}.) are shown here as well as the "protractor hyomandibularis" muscle (*pt.*, *hm.*). Teeth (*t.*) are seen as far down, inside the throat, as the first hypo-branchials.

Section 14.—In this section (Plate 15, fig. 6), the hind-brain (C^3.) is much smaller, but the auditory capsules are at their widest part; they are continuous with the thickening parachordals, which enclose an enlarging notochord (*iv.*, *nc.*). Below these parts and their underlying vessels, teeth (*t.*) are still seen. The super-occipital tegmen (*s.o.*) is growing inwards from the edge of the capsules, but these margins of the great fontanelle are nowhere closed in (from end to end) at present.

The arch of the anterior and horizontal canals, and the vestibule (*a.s.c*, *h.s.c.*, *vb.*) are laid open, and below, in the great open "meatus internus," the ganglion of the 7th and 8th nerves is still seen.

The hyomandibular (*hm.*) is here articulated by its convex head with the concave surface of the auditory capsule, and looks like the proximal segment of a large limb joined to its own limb-girdle. This is correlated with the downgrowth of the mouth, whose own arch has been carried far away from its cranial attachment. Here we see the symplectic, inter-hyal, and proximal part of the cerato-hyal (*sy.*, *i.hy.*, *c.hy.*); and behind these, near the middle, the lower part of the first and second branchial arches ($h.br^1.$, $c.br.^2$, $h.br^2.$, *b.br.*).

Section 15.—In this section (Plate 15, fig. 7) the posterior canal (*p.s.c.*) is laid open throughout its whole extent, and here the capsule (*au.*) is re-acquiring an inner wall. Below, it is joined to a very solid basal plate (*iv.*), right and left, the moieties of this plate clip the large notochord (*nc.*), which is only partly embraced by them.

Parts of the first and second branchial arches ($p.br^1.$, $e.br^1.$, $c.br^1.$, $c.br^2.$) and some gill papillæ come into view here.

Section 16.—In this view (Plate 15, fig. 8) the back wall of the auditory capsule (*au.*) behind the posterior canal is shown; here the massive basal plates (*iv.*) are growing upwards to form the occipital arch (*e.o.*), and are separated from the auditory capsules by a large chink. Here the 9th and 10th nerves (IX., X.) are seen growing from the hind-brain ($C^3.$), forming their ganglia and giving off their trunks. Part of the gill arches (*br.*) and their gills are shown on each side of the pharynx.

Section 17.—Here we see (Plate 15, fig. 9) that the occipital arch (*e.o.*) is imperfect above; below, each mass of cartilage (*iv.*) cleaves closely to the huge notochord (*nc.*) the arch is produced into the angular processes on each side that project from the auditory capsules (see fig. 13).

Fourth Stage.—Young Sturgeons (Acipenser sturio), 7½ *to* 8 *inches long.*

In this stage the Fish is completely metamorphosed, and the only important change which takes place afterwards is immense increase in size, and the addition of certain bony centres, both parosteal and ectosteal.

I have had no intermediate sizes between *Sterlets* 7 lines long and *Sturgeons* 7 inches long, but in *Lepidosteus*, another Ganoid (the subject of my next paper), two instructive stages come in at this point, and make the interpretation of this lower type of skull easy.

In larval *Lepidostei* the size of my largest larval Sterlets, namely, about 15 millims., the azygous intertrabecula has already filled in the space between the trabeculæ, in front; and in specimens already like the adult, and 1 inch long, the trabeculæ have developed their cornua, and the intertrabecula has shot forwards as a long pre-cornual rostrum. At that stage the endocranium of *Lepidosteus* is extremely Acipenserine, and explains, and is explained by, the skull of this stage in the young Sturgeon.

A. *Ectocranium of the young Sturgeon* (Acipenser sturio).

In this stage there are scarcely any proper "parostoses" and very few "endostoses," but the head is well covered with "dermostoses" that are simply the ordinary Ganoid scutes (Plate 15, figs. 10–12), brought more or less into relation, both in *form* and in *number*, with the underlying endo-skeletal structures.

Of course, the homology of these scarcely altered superficial scutes with the special deep laminæ of bone that are so completely dominated by the endocranium in higher types, is imperfect and partial. These scutes present us with *too much* or *too little* when we are looking for the normal, highly specialised "investing bones" of the higher types; their inner layer, only, can correspond with those bones, and that is but imperfectly related to the parts within. Moreover, it is only in certain regions that any strict comparison can be made: this is in those cases where some unusually large scute has starved out its neighbours and has become the roof or wall of some particular part of the skull or face. Nevertheless, for the sake of uniformity of language, I shall call that scute which more perfectly than any other covers the nasal capsule, the *nasal* (*n'.*); the large plates over the hemispheres, the *frontals* (*f'.*); those over the mid-brain, the *parietals* (*p'.*); and those over the auditory ledge, the *squamosals* (*sq'.*).

In the face it will not be difficult to seize upon the true meaning of certain ichthyic bones, namely, the "operculars;" and the "splints" (parostoses) that are applied to the highly specialised pterygo-quadrate and mandibular apparatus will also, by comparison with like parts in other Ganoids, and in the *Teleostei*, be interpretable.

The round swollen head of the larva (Plate 12) is now changed into a long wedge-shaped recurved rostral structure (Plate 15, figs. 10–12), and this structure is invested now by solid ganoid plates in great number; but only certain of these can be pitched upon as deserving a special name.* The number of bones covering the snout is very great; none of these can be called "premaxillary," only one can even by courtesy be called "nasal" (figs. 10, 11, *n'.*), whilst below (fig. 12) several bones contend for the name of "vomer." The eye is protected by supra-, post-, and sub-orbitals (*s.ob.*, *pt.ob.*, *su.ob.*); the post-orbitals run back and become temporal scutes, the chief of which is called the squamosal (*sq'.*). A very fine scute lies over the opercular region and is the true opercular (*op.*); under it there are two rugged, squarish plates—these are the sub-opercular and inter-opercular (*s.op.*, *i.op.*); the pre-opercular (Plate 16, fig. 1, *p.op.*), as in *Lepidosteus*, is a very small "parostosis," applied to the side of the quadrate region of the suspensorium.

The orbital rim and eye (fig. 10) are small; the nasal pouch has a small upper, and a large lower opening; these openings are obliquely placed, so that the upper is also the foremost space. These capsules keep close to the antorbital region; in *Lepidosteus* they are carried to the end of the long beak. Behind the main post-orbital, and at cartilages appear right and left of the notochord. That rod (*nc.*) is invested with a thick mesoblastic sheath of true cartilage, which, as the sections show, is thoroughly confluent with the parachordals in the basis cranii. In front (Plate 16, fig. 2, *nc.*, *p.cl.*, *py.*), it runs up nearly to the pituitary space, but does not ascend into the post-clinoid wall—an arched and almost horizontal plate of cartilage, the *true organic end* of the skeletal axis ; all the rest, to the end of the snout, is formed of special outgrowths from the fore end of the basal plates. The relative size and thickness of the various parts of the chondrocranium will be shown afterwards in the description of the sections.

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* I must refer my readers to the views of those excellent "experts" whose works are referred to in the Bibliographical List, especially to those of Professors Huxley, Traquair, and Bridge.

C. *Visceral arches of a young Sturgeon 7¼ inches long.*

There is no *distinct* rudiment of any arch in front of the mandibular, with its extended and complex pterygo-quadrate "pier;" the rudiment of the ethmo-palatine cartilage only exists as an extension of the aliethmoidal mass (Plate 16, fig. 1, *al.e.*). The pterygo-quadrate plate (Plate 16, figs. 1, 4, 5, 6, *pg.q.*) lies in an almost horizontal plane, and at a very variable distance from the basis cranii. The right and left plates meet by their extensive straight upper edge; then they curve outwards and backwards; their fore margin is rounded, their inferior edge concave, and their hinder edge is sinuous and notched. Outside the rounded condyle (*q.*) there is a leafy growth, imperfectly adze-shaped, which passes outside the adductor mandibulæ; this is the "orbitar process" (Plate 16, figs. 1, 7, *or.p.*). Three-fourths of their inner face is invested by the pterygoid bone (*pg.*), a thickish plate with a deep gap in its blade, in front; the front third of the cartilage is bare, and its hinder margin has suffered absorption--not direct ossification, through the pressure of the pterygoid bone. A sharp style of bone, with its broad end in front, lies along the concave antero-inferior margin; this is the palatine (*pa.*)—a mere parostosis.

A larger bone curves round the front of each plate, where the two sides meet—both bone and cartilage—and then runs backwards, outside the adductor mandibulæ, and is attached to the outer face of the orbitar process; this is the maxillary (*mx.*). Mounted on the hind part of the maxillary, and at right angles with it, there is a little triangular bone, with its apex upwards; it binds against the ribbed outer edge of the quadrate region of the cartilage; this is the pre-opercular (*p.op.*), smaller, here, than in *Lepidosteus.*

The mandible (*mk.*) is shorter than the forwardly extended pier, it is like that of a Tadpole, having a thick articular region, a hollow for the quadrate, a rounded angular process, and a short terete main rod; a flat dentary bone (*d.*) invests its outer surface, which has a similar outline to the rod.

The most remarkable part of this apparatus, however, is the *common compound* (tesselated) "metapterygoid" region. At first (Plate 13, fig. 10) the main middle piece, only, was present; then a right and left segment appeared (Plate 15, fig. 1); now (Plate 16, figs. 1, 5, 6, *mt.pg'.*, *mt.pg''.*) there are fifteen. These have a general symmetry, the large median piece being sub-oval and the large lateral pieces wedge-shaped; but the number of the lesser segments varies, and the outermost, on the left side, is only half separated. In front, the edge is turned downwards (Plate 16, fig. 6), and here a second median cartilage appears, with three lesser segments on the right, and two on the left side. The whole of this "unpaariger Gaumenknorpel" (MÜLLER) is a gently convexo-concave, two-winged structure, which finishes the hard roof of this remarkable tubular protractile mouth.

The uppermost segment of the hyoid arch (*hm.*) is nearly twice the height of the ascending, but arrested, quadrate region; it is the largest of five "internodes" in this double-sized, forked arch, with its double function. The head of the hyomandibular is rounded (Plate 15, fig. 13, and Plate 16, figs. 1, 5, *hm.*); it curves backwards, and sends from its lower two-thirds a large thick flange, the opercular process; this causes the width of the bar to be more than doubled. Its lower condyle is cylindroidal, and the concavity on the next joint answers to it, so that it is like the humero-ulnar joint of a Mammal. Just the neck of the bar is ringed with an *ectosteal* deposit. The next segment, or symplectic (*sy.*), is scarcely half the bulk and length of the last, and its shape is different; it is a phalangiform cartilage, hinged to the hyomandibular, above, tied by ligament to the quadrate and angle of the jaw, antero-inferiorly, and having a little concave facet inside its upper part for the inter-hyal (*i.hy.*), a very small subquadrate segment.

This small *secondary* suspensorium carries the lower part of the arch so that it lies inside the upper; it is composed of two segments, for the distal fifth is segmented off; the main piece is the phalangiform cerato-hyal (Plate 16, figs. 1, 4, 5, *c.hy.*). The middle third is ossified; the distinct piece at the end, the semi-oval hypo-hyal (*h.hy.*), is soft; they meet by their narrow rounded ends, without the intervention of a basi-hyal.

The rest of the arches—the five branchials (Plate 15, fig. 13, and Plate 16, figs. 1, 4, 5)—are very uniform, very solid, grooved on their outer faces for the branchial vessels, and are quite unossified at present, and at present they have only one common basal bar, the basi-branchial (*b.br.*), which does not reach the last arch. They form, below (Plate 16, fig. 4), a very regular series with the lower part of the hyoid arch; they are, in their lower part, larger than the cerato-hyal at first, and then lessen, backwards.

Only the first three have hypo-branchial segments (*h.br.*); these are larger than the hypo-hyal. The upper part of each arch is shorter than the lower; in the first four there is a single small ear-shaped pharyngo-branchial (*p.br.*); thus the last arch has only one piece on each side, and the last but one, three. The segmentation of the hyoid arch is thus seen to be very different from that of a typical branchial, which has no *interbranchial* piece, and the upper part of which is directly superimposed on the lower; moreover, I look upon the hyomandibular and symplectic as a divided *epi-hyal*, with no *pharyngo-hyal*, above.

D. *Transversely-vertical sections of the head of a young Sturgeon 8 inches in length.*

These sections show much clearer signs of the composition of the great ethmo-nasal tract than can be got from the outside views of dissected skulls; moreover, they display the curiously cancellated and burrowed condition of the very hard hyaline cartilage. There is generally some fatty tissue imbedding the vessels that run in these burrows and spaces.

Section 1.—The first of these (Plate 15, fig. 14) is through the end of the beak, and the "prenasal rostrum" or intertrabecula (*i.tr.*) is depressed or spindle-shaped in section; rough dermal bones are seen surrounding it.

Section 2.—In the next (Plate 15, fig. 15) the cornua trabeculæ (*c.tr.*) are cut through at their fore end; they are depressed at this part, sub-concave above, rounded below, and attached by a narrow isthmus to the top of the intertrabecula (*i.tr.*) which is sub-triangular here, and dilated and convex below. A rounded chink separates these three pre-cranial elements almost to their roots. The dermal bones form a flat upper, a round lower surface, and a lobate side.

Section 3.—Here (Plate 15, fig. 16) the three elements have become fused together, but the burrowing vessels and the fatty tissue show the original line of union of the parts. The form of the triple rostrum is, above, concave at the middle, and convex at the sides; below, concave laterally, and convex at the middle.

The dermal covering takes the same form, but the sides are notched; here, as in the last, a sub-marginal groove, right and left, appears, below.

Section 4.—Here (Plate 16, fig. 8) the enlarging beak shows a more rounded cartilaginous pith, but is very similar to the last; this is more than half way to the nasal capsule, behind.

Section 5.—In this (Plate 16, fig. 9) the rostrum, close in front of the nasal capsules, has become multiangular; the cornua trabeculæ (*c.tr.*) are again united merely by an isthmus to the intertrabecula mass; they are thick, almost bilobate, and descend obliquely; they are separated by a semicircular notch, laterally, from the middle part (*i.tr.*); below, also, a similar concavity is seen, right and left, but twice as large as those on the sides. Between the latter the intertrabecula is a rounded beam; above, it is concave, and on each side has a lateral angle which bounds the lateral notch. The bones outside carry out this ridged form; below, a thick squarish mass of bone is seen, which is one of the vomerine series (*v.*).

Section 6.—Here (Plate 16, fig. 10) the olfactory sacs (*ol.*) are cut through their middle, and here the angulation of the parts is intensified; the cornua trabeculæ (*c.tr.*) are now large wings, pedate at their free ends, and are separated laterally from the intertrabecular mass (*i.tr.*) by a very large semi-circular notch, in which the nasal capsule lies. Here the intertrabecula forms a roof over the sac, right and left, and it is very hollow above; below it is burrowed by a vomerine scute (*v.*), and is becoming narrower, and more like a distinct beam; here also the outer bones do but fringe and enlarge the section.

Section 7.—In this (Plate 16, fig. 11) section the antorbital wall is exposed, behind the nasal sacs; that wall is composed of the aliethmoids (*al.e.*) and the ethmo-palatines (*e.pa.*). Here the paired cartilages are the trabeculæ (*tr.*) for this section is behind the cornua; but the intertrabecula (*i.tr.*) is very large from top to bottom. The middle region of the intertrabecula is the perpendicular ethmoid (*p.e.*), on each side of which the olfactory nerves (I.) are escaping; its alæ above are becoming the superorbital bands (*s.ob.*), whilst below, its huge beam is burrowed by a vomerine bone (*v.*).

Section 8.—In this section (Plate 16, fig. 12) the eye-ball (*e.*) is just caught, and the cranial cavity laid open at the olfactory foramina, with their nerves (I.); the rest is very similar to the last section; in both, dermal bones defend the sides of the face, and above, the frontal scute is cut through.

Section 9.—This (Plate 17, fig. 1) is through the middle of the orbit, where the superorbital band (*s.ob.*) narrows in; here the supercranial valley is very large and deep, and the section of the skull has the shape of an Ox's face. In this young specimen the chondrocranium is very massive, and the pyriform cavity for the hemispheres (C^{1a}.). is only one-fourth as large as the section itself, which is widest below, and rather pinched in the middle. The skull, below the cranial cavity, is as deep as the cavity, and is burrowed, below, by the splintery fore end of the huge parasphenoid (*pa.s.*); the swelling cartilage on each side belongs to the trabeculæ (*tr.*) the middle part to the intertrabecula (*i.tr.*). The orbital muscles (*or.m.*) are planted in chinks of the basal mass.

This section is in front of the mouth and through the front of the upper lip (*u.l.*); there is, here, a crescentic cavity, with the horns below.

Section 10.—In the next section (Plate 17, fig. 2) the small eye-ball is just missed, but the optic nerves (II.) are seen emerging from the brain (C^{1a}.). Here the cavity is something like an hour-glass, being as wide below as above; the tegmen cranii (*t.cr.*) is twice as thick as in the last, and the basal mass (*tr.*, *i.tr.*) only half as thick, for this is behind the lobes that envelop the parasphenoid (see Plate 16, figs. 1–4).

Here the superorbital bands are wider and more solid, and externally, a new plate of cartilage has come in, on each side from the eaves of the hind skull; this is the sphenotic (*post-frontal*) lamina (*sp.o.*). The solid basal plate is grooved sub-laterally and in the middle, and the parasphenoid (*pa.s.*) fitting to these sinuosities is, in section, like a stretched bow.

Below the base of the skull there is a quantity of very lax tissue, permitting the greatest freedom to the movements of the protrusible oral apparatus, and below this we see the large arched mass of the "adductor mandibulæ" muscle (*ad.m.*) on each side. Below the muscles, the fore part of the pterygo-quadrate cartilages (*pg.q.*) are cut through, and they are flanked by the maxillary and palatine bones (*mx.*, *pa.*) the former outside, and the latter beneath.

The mouth is nearly closed, the upper and lower lips, as in the next section (*u.l.*, *l.l.*), are seen with their rugæ, but without their teeth, and in the lower lip the mandibles (*mk.*) are just brought into view.

The parietal scutes were cut through above, and post- and sub-orbitals were seen, at the sides, and below, but not figured.

Section 11.—In this section (Plate 17, fig. 3) the sphenotic lamina (*sp.o.*) is thicker, but is still separated by a notch from the tegmen cranii (*t.cr.*); that part is thinner in the middle, and deeply concave. Here the cranial cavity has widened, for the section is close in front of the mid-brain. The section, now, is that of an Ox face with drooping horns, and the muzzle is now narrower around and under the infundibulum (*inf.*). On account of the very small size of the eye-ball (Plate 17, fig. 1, *e.*) *four* of these sections are between the eye and the ear; in this the orbital wall (*o.s.*), which is extremely thick, is twice scooped for the orbital muscles and the fatty masses that occupy the orbit—much too large for the eye-ball. Here the intertrabecula has died out, and the trabeculæ (*tr.*) have united directly with each other, and the plate thus formed has lost half the thickness seen in the last section (Plate 17, fig. 2, *tr.*, *i.tr.*).

The primary form of the trabeculæ is still seen at the sides, below, and the whole of the basal plate is crenate, making the parasphenoid (*pa.s.*) assume an undulated form as it follows the risings and fallings of the cartilage. In this section the oral apparatus is cut through the middle, and the opening itself is crescentic below, with a notched upper outline; this is due to the projection right and left of the pterygo-quadrates (*pg.q.*) and their investing bones, the palatines (*pa.*, *pg.*).

Here, the cartilages covered by the large adductor muscles (*ad.m.*) are sigmoid in section, and besides the outer bones, the pterygoids (*pg.*) now show themselves, both as an inner and an outer section, being through their forks. The quadrate region, with its orbitar process (*q.*, *or.p.*) is separate, now, and over it is the maxillary (*mx.*); to the quadrate the mandible (*mk.*) is articulated, it then passes almost directly inwards towards its fellow, which it does not quite meet; the dentary (*d.*) is seen flanking the Meckelian rod.

Section 12.—In this section (Plate 17, fig. 4) the cranial cavity is Y-shaped, and contains the front of the mid-brain above the fore-brain (C^1.), with the infundibulum passing into the pituitary body (*py.*). Here, in front of the post-clinoid wall, the alisphenoidal region (*al.s.*) is extremely thick —twice as thick as the tegmen (*t.cr.*) and the continuous sphenotic wings (*sp.o.*).

In this section the parts round the mouth are like those of the last, but the foremost azygous metapterygoid (*mt.pg′.*) comes into view, and the mandibles (*mk.*) are cut away, distally.

Section 13.—This (Plate 17, fig. 5) is through the middle of the mid-brain (C^2.), and the back of the pituitary body (*py.*). The post-clinoid wall, which is an oblique shelf running forwards and a little upwards (Plate 16, fig. 2, *p.cl.*), is here cut through obliquely so as to appear thicker than it is actually. This section seems to show the pituitary body as lying in a separate and very solid box, for the rest of the skull is quite distinct from it, above. This arises from the fact that the foramina ovalia are here, with the trigeminal nerves (V.); the alisphenoidal wall, the roof, and the thickened sphenotics are seen in the upper part of the section. The parasphenoid (*pa.s.*) curls round the flatter base, the parietal (*pg.* by mistake for *p'.*) and post-orbtial scutes were cut through. Below, the adductor mandibulæ muscles (*ad.m.*), the pterygoid cartilages and bones (*pg.q.*, *pg. q.*, read *pg.*) are severed, and the latter are in one piece, for this is behind their great notch. The smaller azygous metapterygoid (*mt.pg'.*) is seen; the extreme angle of the quadrate (*q.c.*); and the hinge and part of the shaft of the mandible (*mk.*) through the dentary and the pre-opercular bones.

Section 14.—Here (Plate 16, fig. 6) the breach in the lower part of the walls is repaired, for this is behind the foramina ovalia, and through the fore part of the auditory capsules (*au.*); the ampulla and front part of the arch of the anterior canal (*a.s.c.*) are laid open. The roof is thicker and flatter; the sphenotic wings (*sp.o.*) are still large and thick; the cavity of the skull is single, narrower, and is half a long ellipse in shape, the angles of the broad upper end being rounded. Here the notochord is not seen, it has retreated too far backwards, but the parachordal mass (*iv.*) is hugely thick, and is wider; it is close behind the post-clinoid wall. The fore part of the hind-brain (C^3.) is cut across, where it is giving off the facial nerves (VII.), close behind the roots of the trigeminals (fig. 5, V.). The last section was cut through the interspace between the pterygoid band and the orbitar process (fig. 5, *pg.q.*, *q.c.*; and see also Plate 16, figs. 1 and 7); here the back of the suspensorium (*q.c.*) is cut through, at the hind part of the maxillary bone and the orbitar process (*mx.*, *or.p.*). A notch seen in the hind margin of the pterygo-quadrate cartilage (Plate 16, fig. 5, *pg.q.c.*) is cut across here, and the median part of the cartilage crops up, again, right and left; between these the main azygous metapterygoid (*mt.pg'.*) is seen. The angle of the mandible (*mk.*) and the end of the dentary (*d.*) are also cut across.

Section 15.—In this section (Plate 17, fig. 7) the chondrocranium attains its greatest solidity, and here the small cranial notochord (*nc.*) has its point cut through. The sphenotic has given place to the "pterotic" region, the arch of the anterior and the ampulla of the horizontal canals (*a.s.c.*, *h.s.c.*) are exposed, and so also is the auditory nerve (VIII.), as it arises in the hind-brain (C^3.) and runs through the meatus internus into the vestibule (*vb.*). Here the tegmen cranii is thick and concave, and the pterotic expansions (*pt.o.*) are very rough and lobulate. Under the bulging sinuous parachordal mass the parasphenoid (*pa.s.*) has become very wide, and now sends down its free edges. Under the capsules the "protractor hyomandibularis" muscles (*pt.hm.*) are seen as huge triangular masses, and the "adductor mandibulæ" muscles (*ad.m.*) are cut through in their hinder part. A little of the pterygoid region (*pg.q.*) and the hind angle of the quadrate (*q.c.*) come into view, and between the former the median and main lateral metapterygoids (*mt.pg'.*, *mt.pg''.*). The deepest part of the pterygoid bone (*pg.*) is seen flanking the inner face of the suspensorium. Under the quadrate a new and thick cartilage appears; this is the symplectic (*sy.*), it is crescentic in this, its front edge (lower end), and between it and the quadrate there is a small nucleus of cartilage which will be described in the next stage—it is a "suspensorial ray" (*sp.r.*). The chief scutes seen are the parietals; the squamosal, the opercular, and the sub-opercular, also, may be traced at this point, but are not figured.

Section 16.—In this slice (Plate 17, fig. 8) we see several new parts come into view below; above, the skull is altogether flatter, and the supercranial valley narrower. The auditory nerve (VIII.) is still shown, also the arch of the anterior and horizontal canals (see also fig. 7, *a.s.c.*, *h.s.c*). The basal plate (*iv.*, *nc.*) is very irregular, as it passes into the auditory capsules, right and left; the parasphenoid (*pa.s.*) partakes of this irregularity, and here its decurved wings are larger. The compound metapterygoid plate is composed at this hind part of a large middle, two large lateral, and two lesser intervening plates (*mt.pg'.*, *mt.pg".*). The top of the hyomandibular is just seen and most of its great protractor muscle (*pr.hm.*); below, the symplectic is cut through from top to bottom. Below the wide oral cavity the cerato-hyal (*c.hy.*) with a thin ectosteal coating, the hypo-hyals (*h.hy.*), the fore part of the basi-branchial (*b.br.*), and the fore end of the first hypo-branchials ($h.br^1.$) are also seen. The scutes are very similar to those of the last.

Section 17.—In this (Plate 17, fig. 9), the cranial valley is seen to be wider again; the auditory sacs are cut through where the posterior canal (*p.s.c.*) is descending, and the whole structure is lessened laterally; a thin pterotic eave (*sp.o.*, read *pt.o.*) grows down on each side. Here the hind-brain (C^3.) is less, and it is giving off the roots of the large vagus nerve (X.) whose ganglion root and stem are laid bare. Here we still see that the auditory sacs are open to the skull (or only closed by membrane), and here the sacculus (*s.*) is seen hanging from the rest of the vestibule (*vb.*). The notochord (*nc.*) is becoming larger, and the thick curved basal plate (*iv.*) is here separate from the auditory capsules; below it the parasphenoid (*pa.s.*) is thick, and at its notched sides we see a pharyngo-branchial (*p.br.*) and outside this part of the gills (*g.*). The lower part of the hyomandibular, part of the symplectic, inter-hyal, and cerato-hyal (*hm.*, *sy.*, *i.hy.*, *c.hy.*) are cut across; and near the mid line the basi-branchial, the first cerato- and hypo-branchials, and the second hypo-branchials (*b.br.*, $c.br^1.$, $h.br^1.$, $h.br^2.$).

Section 18.—In this section (Plate 17, fig. 10) the post-auditory fontanelle is just missed, and we see that it must answer to the interspace between the supra-occipital and first vertebral arch; for the chondrocranium (see Plate 15, fig. 13) sends its copious growths over the proper spinal region. The cavity here is very large, this being, indeed, the "foramen magnum," and the ex-occipital walls are very thick, and almost vertical. The hinder part of the vagus nerve (X.) is still seen in the chink which separates—up to this point the sides form the base, which is increasingly massive, but has not a very large notochordal section (*nc.*) in it. The parasphenoid (*pa.s.*) is beginning to break up at the middle; outside it a pharnygo-branchial is seen (*p.br.*), and outside that the most backwardly projecting part of the massive hyomandibular (*hm.*).

Section 19.—The last of this series (Plate 17, fig. 11) is post-cranial; the ganglion of a spinal nerve (*sp.n.*) is cut across; the medulla spinalis (*my.*) is seen to be very small in proportion to the spinal cavity, and the notochord (*nc.*) is twice as large as in the skull, and reaches the theca vertebralis. The parasphenoid (*pa.s.*) is in two parts (see Plate 16, fig. 3); the hind and transverse processes of the undivided vertebral tract are very large, and are covered with large scutes. This section is behind the operculum (Plate 15, fig. 10, *op.*), and through one of the hinder branchial arches; in the general branchial recess, the pharyngo-, epi-, and part of the cerato-branchials (*p.br.*, *c.br.*, *e.br.*) of a middle gill arch, and some of the gills, are shown.

Behind this part the notochord becomes thicker, and its chondrified sheath more distinct; the forked parasphenoid dies out, and the *neuro-central* cartilages appear (Plate 16, fig. 3).

Fifth Stage.—Adult Sturgeon (Acipenser sturio).*

The Sturgeons dissected for this stage were about 4 or 5 feet long, such as are most commonly seen in the markets; my last stage is from a much larger specimen, but not from the largest. Dr. GÜNTHER ('The Study of Fishes,' p. 362) says that this species attains a length of 18 feet.

The swelling of the fore part of the trabeculæ and intertrabecula in the orbital region becomes very remarkable, and the cartilage becomes subdivided into an upper and a lower stratum; between these the parasphenoid (Plate 18, fig. 3, *tr.*, *i.tr.*, *pa.s.*) grows as a thin dentate lamina, with one very long median spike. The clavate swellings of the trabeculæ bind on the sides of the exposed part of the parasphenoid, and the *second floor* of cartilage in the middle (*i.tr.*) runs up to the base of the huge rostrum, appearing below as a rounded elevation, separated from the lateral lobes by a deep sulcus, which is filled with fatty tissue. In the fatty tissue outside the paired lobes of cartilage there is a series of four thin cartilaginous plates (*e.pa′.*); they are outside the trabeculæ, and behind the ethmo-palatine boundary of the nasal sac, and probably represent, partly, as Mr. HOWES suggests, the free ethmo-palatines of other kinds; in Siredon I have shown that there are two of those cartilages on each side (Phil. Trans., 1877, Plate 24, figs. 1–3, *e.pa.*, *pt.pa.*)—an "ethmo-palatine" and a "post-palatine."†

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* The dissections, figures, and notes for this stage were made for me by my friend Mr. GEORGE HOWES, Professor HUXLEY'S talented Demonstrator.

† I wish to put all these *pre-oral* growths together for comparison, in hope of obtaining a clue to their real nature. Mr. BALFOUR'S writings have made me very doubtful of the view that, at one time, seemed to me to be almost demonstrable—viz., that these antorbital cartilages were in reality the rudimentary upper part of a *pre-mandibular visceral arch*, but their position, in front of the hypoblast, makes their determination as difficult as that of the *pro-chordal* part of the basis cranii.

The most important further modifications to be seen in these large individuals, are in the visceral arches.

There are seven arches, and the whole series is displayed in a side view (Plate 18, fig. 5) so as to show their relative size, and their varied modification; in that figure the inferior part of the hyoid arch is shown as drawn backwards towards the first basi-branchial (*c.hy.*, *b.hy.*, *b.br*^{1}.); in reality it runs across to meet its fellow in the floor of the mouth (fig. 10, *c.hy.*, *h.hy.*).

The two first arches, the mandibular and hyoid, are intensely specialised; the other five are very similar to those of the Elasmobranchs, save that they have some of the larger bars partly ossified; and are still more like the branchial arches of Holostean Ganoids and Teleosteans. The upper elements of the mandibular arch, the right and left "suspensoria," are transformed into the oblique convex roof of the very mobile protrusible mouth, and the free mandibles are made to be antagonistic to the antero-inferior part of this complex structure. Each cartilaginous suspensorium is a broadly falcate plate, whose arched upper border comes in contact with that of its fellow in front and above; below and behind, it suddenly bends forwards and becomes very solid, to form the quadrate condyle (figs. 4, 5, *q.c.*). The whole plate is gently and sinuously convex above and concave below; behind, over the hinge, it thickens into a rib-like enlargement, and grows outwards and forwards as a superficial "orbitar process" (*or.p.*). The upper margin is arched regularly until near the hinge, and then is hollowed a little; the lower margin is twice-notched and concave. The bones applied to these pterygo-quadrate plates have not caused the absorption of the cartilage in this stage, which is *relatively* more extensive than in the last stage, in which the hind margin was notched. The outermost bones are the largest; they are the maxillaries (*mx.*), they are strongly bowed, dilated most where they meet in front of the cartilaginous plates, and to a lesser degree where they bind upon the orbitar processes behind. A large space for each "adductor mandibulæ" muscle exists between the maxillaries and the pterygo-quadrate cartilages. Another superficial bone stands upright on the hind end of the maxillary; it is a high triangle, and its base is below. It binds on the thick, ribbed, outer edge of the suspensorium in its quadrate region; this is the small "pre-opercular" (*p.op.*), whose almost equally small counterpart exists in *Lepidosteus.* Along the lower edge of the cartilage, in front, a very jagged little bony scale is seen, overlapped by the fore end of the maxillary. This is the mesopterygoid (*ms.pg.*); it was not a separate bone in the last stage. Behind it there is a small style, with its sharp end behind, reaching to the end of the foremost and larger notch; this is the palatine (*pa.*). Inside the plate (*pg.q.*) there is a large parostosis, the pterygoid; it lines all the cartilage except a falcate tract in front and above; this causes it to be sharply notched in front; this form is well shown by the shading of the cartilage (Plate 18, fig. 4).

The mandible or Meckelian rod (Plate 18, figs. 5 and 8, *mk.*) is a very short stout cartilage like that of a Tadpole; the condyle is convexo-concave, and the angular

The neck and shoulders of the hyomandibular are ossified, so as to leave the round convex head and expanded lower part unchanged. The osseous shaft is hour-glass-shaped, very thick, and leaving only a small core of cartilage unossified (Plate 18, figs. 5–7, *hm.*); this bony tract is very angular, in section, and somewhat four-cornered. On the outside of the bony sheath, embedded in the periosteum, there are simple and forked cartilages (Plate 18, figs. 5, 6, 7, *hm.r.*), evidently rudimentary "branchial-rays;" as seen in the sections (figs. 6, 7) they are very solid, "and usually exist upon (*a*) the anterior, and (*b*) the outer face only; in one specimen, however, (fig. 7) they also occurred upon the anterior and inner" (HOWES); they are very variable as these sections show.

The "toe" of the hyomandibular (*hm.*) rests upon the upper face of the upper end of the next segment, the symplectic or lower epi-hyal (Plate 18, fig. 5, *sy.*); the two are attached together by a strong fibrous joint. This distal segment of the hyoid "pier" is half the length and half the medium width of the upper piece; it is phalangiform, straight, and thick. Distally it is attached by a strong fibrous joint to the quadrate region (*q.c.*), which it carries; proximally, at its proper upper end it is articulated to the inter-hyal (*i.hy.*); it is wholly unossified.

The intercalary segment, or "inter-hyal" (*i.hy.*) is a sub-cubical, lobulated mass of cartilage, interposed between the symplectic and the lower element of the hyoid arch, the cerato-hyal (*c.hy.*). This latter segment in the undisturbed condition of the parts runs across and a little forwards under the throat; it is about the size of the symplectic, but more slender and has a narrow waisted ectosteal tract a little below its middle. This bone is nearly half the height of the bar, is pinched and angular, has an unossified pith and extra-hyal cartilages (Plate 18, fig. 9, *c.hy.*, *c.hy.r.*), like the hyomandibular.

There is a thick wedge of cartilage segmented off from the cerato-hyal, the hypohyal (*h.hy.*); it is attached by fibrous tissue to its fellow, the related ends being narrow, and also to the fore end of the first basi-branchial (Plate 18, figs. 5 and 10, *h.hy.*, $b.br^1$.).

The thick, solid, strongly bent branchial arches (Plate 18, figs. 5 and 10) lessen and become simpler from before backwards. The joint between the main upper and lower segments (*e.br.*, *c.br.*) is a well formed "elbow," the upper part is a little less than the lower. Only the first and second branchial arches are perfect, and they have acquired an additional pharyngo-branchial (fig. 5), and these small segments rest on short forks of the epi-branchial (*e.br.*). There is only one pharygno-branchial on the third arch ($p.br^3$.), and that on the fourth has (evidently) become fused with the epi-branchial ($e.br^4$.). The fifth arch is in one piece, it is a cerato-branchial (fig. 10, $c.br^5$.), and like the four cerato-branchials in front of it, has a bony tract in its middle part. Only the first and second epi-branchials ($e.br^1$., $e.br^2$.) have ectosteal tracts. On the first of these there are at least two imperfect "rays" (*br.r.*), as on the arches in front. The lower part of each arch is bowed outwards, the upper is sigmoid, and they are very angular, especially where the ectosteal tract exists. Only the first three have hypobranchials (Plate 18, figs. 5 and 10, *h.br*$^{1-3}$.); these begin much larger than the hypohyal and then lessen backwards; they are flat, finger-shaped, and not ossified.

Another addition of cartilaginous segments has taken place below; in the young Sturgeons (Plate 14, fig. 5, and Plate 16, fig. 5) there was only one basi-branchial carrying the three first arches. Now, *three* new smaller segments have appeared (*b.br*$^{4-6}$.), the last of these is wedge-shaped, and belongs to a suppressed arch, for it passes behind the pedate end of the arrested fifth arch; the foremost piece belongs to three arches.

These piers are compressed, and the first, which belongs to *three* arches (*b.br*$^{1-3}$.), is nodose.

Sixth Stage.—Skull of an unusually large[*] *Sturgeon* (Acipenser sturio) (*in the Hunterian Museum*).

This very valuable specimen of a much older individual, shows some very important modifications, which have, as it were, *an upward look* towards the Holostean Ganoids and the Teleostei.

In the last stage the pterygo-quadrate cartilages were even more perfect than in the young of 7 or 8 inches in length. In this specimen, however, the "parostoses" have caused a considerable amount of absorption of the hyaline cartilage, just as the dentary, normally, causes MECKEL'S cartilage to shrink and even become absorbed; but I cannot find any evidence of a direct *ectosteal* relation of the bone to the cartilage. The pterygoid bone (Plate 17, figs. 12, 13, *pg.*) gets over to the *outer* side over the convex inner margin, and causes the absorption of the cartilage in that region. Moreover, the jagged bony tract which represents the mesopterygoid, and which in the last was at the antero-inferior edge of the cartilage, is now (Plate 17, figs. 12 and 13, *ms.pg.*) a sharp wedge of bone filling up the space between the forks of the pterygoid on the inner side (fig. 13), and is seen as an oval plate of bone nearer the top than the bottom, on the outer side (fig. 12, *ms.pg.*). These differences are not all due to *age;* there is a considerable amount of variation in individuals of the same age in these types, where the sub-cutaneous and sub-mucous bony tracts are but *deeper scutes,* and where the chondro-skeletal regions are so generalised and, as it were, *hypertrophied.*

With regard to the *buried scutes,* we see a right and left variation in this same specimen (Plate 18, figs. 1 and 2, the *right* and *left* orbital region in this large skull).

Here there are no proper *ectosteal* prefrontals (ecto-ethmoids), orbito-sphenoids, alisphenoids, or prootics; but, since the last stage, parosteal tracts have appeared in all those places; they are *deep* "scutes," not *shallow* endo-skeletal elements, and are the prophecy, so to speak, of the special "ectostoses" seen in the Holostean Ganoids and the Teleostei, and in all the types above them. Round the thick antorbital

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[*] Even this specimen was not half the size this species sometimes attains to.

(ecto-ethmoidal) mass of cartilage I find a thin but rough irregular layer of bone, just where the Teleostei have their proper ecto-ethmoid (Plate 18, figs. 1 and 2, *e.eth'.*). Also over the optic foramen (II.) there is a very large splint (*os'.*) applied to the orbital cartilage; it runs upwards and forwards. On the *right side only* (fig. 1, *al.s'.*) there is a much smaller plate in the alisphenoidal region, and *behind* and partly *round* the foramen ovale (V.) a thin semi-annular plate (*pro'.*) representing the prootic of the higher types. But no direct grafting of bone on the cartilage can be seen, and the *affinity* of the bone for the cartilage or, *vice versâ*, of the cartilage for the bone, is here extremely feeble; there is no material interaction; the co-ordinating force, however, has produced a plate of the proper form, and put it ready for use in the proper place.

Summary and comparison with other types.

It is evident that we have in the Sturgeon a form which is practically intermediate between the Selachians and the Osseous Ganoids (Holostei); the form of the larvæ (Plate 12) suggests this view at once. I must again refer the reader to the researches of SALENSKY and BALFOUR on the embryology of this type; my own recapitulation and comparison must be confined to the cephalic skeleton.

A. *The primordial skull.*

We saw that in the larvæ of *Acipenser ruthenus* only about a third of an inch in length (9½ millims.) the "embryonic cartilage" had largely become "hyaline;" that the foundations of the cranium were laid, and the visceral arches were differentiated and becoming quite solid. Here, it would seem, that in so small a larva of so large a Fish—and that Fish lying at the base of the great *archaic* group of the Ganoids—we have a good chance of seeing the primordial vertebrate skull in its utmost simplicity. The fact is, that we have *a confusingly simple* state of things.

In the fore part of the spine, as well as in the whole basi-cranial region, the paired skeletal tracts that lie right and left of the *mesoblastic sheath* of the notochord, the hardening cartilage shows no signs of segmentation or *intercalary vertebral subdivision;* this is just like what occurs in the Selachians (Trans. Zool. Soc., vol. x., plate 35). Moreover, this Chondrosteous Ganoid remains in this condition, as far as I can find, throughout life, and does not acquire the occipito-cervical articulation, so well known in the Selachians, but which in them is a secondary modification of the parts. By careful comparison of all the facts I have been able to gather by observation of many types and at many stages, I cannot help coming to the conclusion that the *normal intercalary vertebral segmentation* with which we are so familiar in the post-cephalic region of the Vertebrata, generally, is *a comparatively late and secondary specialization* in the evolution of this, the highest, group of animals.

If Mr. BALFOUR's suggestion ('Comp. Embryol.,' vol. ii., p. 366) be true, viz.: that the fore-brain with its special optic and olfactory outgrowths is a sort of outgrowth or addition, itself, to the neural axis, then we have to reconsider the meaning of any and every part of the cranial skeleton which may be formed in front of the notochord and of the overlying and overfolded mid-brain.

Thus all the *prochordal* cranial, and all the *pre-mandibular* facial growths of the skeletal cartilage have to be subjected, again, to the severest morphological inquisition under that light—the light of Embryology—which alone can make manifest their true meaning.

If the notochord be the true organic axis of the skeleton of the animal, and if the visceral (or branchial) arches were only developed in relation to the hypoblastic branchial outgrowths of the dilated respiratory pharynx, then it seems to be necessary that we should consider all the skeletal parts in front of those structures as superadded specializations of, or "outgrowths" from, the front end of the proper Vertebrate form.*

In certain types, namely, the Lamprey, the Selachians, the Urodela, and the Anura, the trabeculæ are chondrified before the hinder or parachordal tracts. Nevertheless, the posterior part of the trabeculæ in them is parachordal; the rest, up to the "atlantal" region, may afterwards chondrify separately, as in the Urodela, or continuously, as in the others.

In *Acipenser*, *Lepidosteus*, and *Salmo* I find no difference of time in this matter, and should therefore be inclined to look upon the earlier growth of the trabeculæ as due to the special weight and pressure of the fore-brain in those cases, and as a non-essential modification, just as I consider the later segmentation of the trabeculæ from the investing mass in *Salmo* and *Chelone*, and to a less extent in Crocodiles and Birds, as a non-essential specialization.

Yet the trabeculæ are as truly part of the proper mesoblastic axial skeleton as the fore-brain is part of the proper epiblastic neural axis.

The whole of the *pre-cerebral* tracts of the skull, namely, the cornua trabeculæ and the greater part of the intertrabecula—all of it that lies in front of the exit of the olfactory nerves—I should consider to be mere *outgrowths* or "apophyses" of the cranial skeleton.

The axial skeleton is more aborted in front than the axial nervous system, even if the fore-brain is reckoned as an additional part, for the mid-brain bends completely over upon itself, and the notochord only partially. Yet, as I have shown in *Chelone viridis*, its cartilaginous mesoblastic sheath is continued downwards as solid cartilage below and in front of the end of the notochord. I am inclined to think that the intertrabecula is a breaking out again of that mesoblastic tract (a sort of cranial "spadix") but with its continuity, for a short space, interrupted.

The ventral part of the primordial skull presents as many difficulties as the dorsal; the post-oral visceral (or branchial) arches, which may be both *superficial and deep*, have to be compared with such facial growths as may exist in front of the mouth, and these, if possible, have their relation to the post-cephalic arches determined.

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* For my own part, I am quite content that this should be so, albeit my own descriptive language will have to undergo a considerable amount of evolutional modification, and many things that seemed to me, once, to be clear and certain, made dark and uncertain.

Long ago, to those who knew nothing of Embryology, all this seemed to be easy enough; now, with all the new, increased light from that source, the problem has become extremely difficult, and is certainly not solved, as yet.

I do not see that the superficial cartilages that surround the mouth have any right to be compared, serially, to the arches of the pharynx or of the chest; they appear to me to be the most *archaic* structures in the skeleton—"barbels," "labial cartilages," "nasal valves"—all these appear to me to be lineally descended from the inner supporting tracts of tissue of oral palpi, such as are met with in so many of the aquatic *Invertebrata.* The investing bones of the face may be grafted on such cartilages, but the two things are quite different in their nature. The condition of the visceral arches in this type, both in the larva and in the adult, has led me to reconsider the whole question of the nature of these arches.

There are several things to be considered at the outset, before a comparison is made of the skeleton of the throat, the branchial skeleton, and the skeleton of the chest. The post-auditory part of the cranium has manifestly undergone *secular* shortening, so that the pharyngeal or ventral region belonging to it often extends under the *twice-segmented* spine, whose fore part is, so to speak, intercalary or superadded, and does not correspond with the arches beneath it, which often extend backwards for some distance.

At one time this appeared to me to be an explanation of the fact that the inner (or proper) branchial arches of Fishes are developed beneath the fore part of the spine and the hind part of the basis cranii, whilst the mandibular and hyoid arches, the first and second of the branchial category, often fix themselves to the basal plates of the cranium.

I now strongly suspect this view of the matter to have been a mistaken one; and that it is the *abnormal size* and *special modification* of the mandibular and hyoid arches that make it necessary for those arches to seize hold of swinging points above their normal dorsal region or apex.

I think that the figures I have given of the visceral arches in the larval Sterlet (Plate 13, figs. 6 and 11) will make my meaning plain. Here only one arch is attached to the edge of so much of the parachordal plate as may creep under the auditory capsule; all the other arches are fairly under the head and forepart of the neck.

This normally inferior position of the visceral arches is best seen however in the early embryo of the Skate (see in "Pristiurus," Trans. Zool. Soc., vol. x., plate 35, fig. 4), where the metapterygoid, hyomandibular, and all the pharyngo-branchials, are shown *in situ,* after the whole cranium had been removed from above.

The sub-division of the pleuro-peritoneal cavity by the hypoblastic branchial pouches in the early embryo, the rapid closure of those cavities (the "head-cavities"), and the relation of the skeletal bars of the pharynx and mouth, whether deep or superficial—all these things have to be carefully reflected upon before any classification of these ventral arches can be made.

It seems to me that a cartilaginous arch developed *inside* a recently-closed "head-cavity" must be a part of the "splanchno-pleure," and that a cartilage developed outside such a recently-closed cavity must belong to the "somato-pleure." *

The *ribs* belong to the outer lamina of the body-wall of the embryo, so also, it appears to me, do the "extra-branchials" of the Lamprey, the Tadpole, and the Shark.

If all this be true, the normal "intra-branchial" arches have no counterparts whatever in the skeleton of the trunk; they culminate in the class of Fishes, and are imperfectly developed, not only in the Abranchiata, but also in the degenerate Marsipobranchii, and the metabolic Amphibia Anura.

The extra-branchials of some of these latter types, and of the Sharks, have some right and title to be classified as a sort of cephalic ribs, but perhaps that claim had better not be pressed for the present; it is safer for the Morphologist to keep certain things in solution, when any doubt remains, than to crystallize them into what may turn out to be obstructive error.

In the metamorphosis of the larval Sturgeon the additional parts are easily explained, and are, for the most part, due to mere increase of certain tracts of tissue, and super-additions of secondary nuclei of cartilage, and of various centres of ossification.

Moreover, the fact that the dermal scutes are largely dominated by the cartilaginous endoskeletal structures of the cranium and pharynx, however interesting from one point of view, is not of any great fundamental importance.

With regard to the great "shoots" of cartilage that grow out, forwards, from the primary basis-cranii, these are parts that undergo a most extraordinary amount of modification in various types; they are specialised superadditions to the essential skull, of great importance in *Taxonomy*, but of little account in that which is fundamental in *Morphology*.

Comparison with Polyodon.

The skull of the other principal existing Chondrosteous Ganoid type comes singularly near to that of the Acipenserine skull, and in some things is curiously unlike it. Near as *Polyodon* approaches in the structure of its skull to *Acipenser*, it differs in having no complex metapterygoid plate, in the feebler ossification of its visceral arches, and in the presence of three pairs of "endosteal" centres not to be found in the Sturgeon. The anterior palatine ectostosis placed just where both the mesopterygoid and palatine plates meet in the Sturgeon, is probably the true homologue of the palatine bones of the Holostean Ganoids; the presence of a small prootic and opisthotic brings us nearer in this case to those higher types. The much larger and more perfect "orbitar process" shows in Polyodon a nearer relationship to the larval Frog; the absence of a metapterygoid plate brings it nearer to the Sharks, and the three pairs of *ectosteal* centres to the Holostean Ganoids. (See BRIDGE, Phil. Trans., 1878, Plates 55–57.)

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* The branchial artery lies *inside* the head cavity, and afterwards, when the branchial arch is developed, runs up a groove on the *outside* of it (see BALFOUR'S 'Elasmobranchs,' plate 14, fig. 13, *a*, p. 208; and 'Comp. Embryol.,' vol. ii., p. 472, fig. 328).

Comparsion with the Selachians.

The development of the basis-cranii and cranial walls in *Acipenser* is very similar to what is seen in the Selachians; and the after modifications are essentially alike, except that in the Sturgeon the cartilage is very massive, and the occipito-cervical articulation is not formed. The separation of a large symplectic, and a lesser interhyal segment, the complex metapterygoid plate, and the partial ossification of the visceral arches, are all modifications that separate this from the Selachian types.

Comparison with the Holostean Ganoids.

Most of the peculiarities which distinguish the skull in that group from that of a Sturgeon are so many steps in the direction of the Teleostean skull. To say nothing of the lighter build of the chondrocranium, the numerous ectosteal patches in the skull wall, the intenser ossification of the visceral arches, and the absence of a distinct symplectic cartilage (it may exist as a separate bony centre, as in the Teleosteans), all these things show how far the Holostei have become specialized beyond the chondrosteous *Acipenser*. An important modification occurs early in *Lepidosteus*, namely, that the palato-quadrate is continuous, at first, with the trabecula; the intertrabecula also appears earlier. I shall compare the skull of the Ganoids, generally, with that of the Teleostei, in my next paper, which will treat of the skull of *Lepidosteus*.

EXPLANATION OF THE PLATES.

Plate.	Fig.	Stage.		Number of times magnified.
12	1	1	Larva (recently hatched) of *Acipenser ruthenus*, 5½ millims. long	17
”	2	1	Larva of same, 6½ millims. long	15
”	3	1	Under view of head of same specimen	15
”	4	2	Head of larva of same, 8½ millims. long.; side view	15
”	5	2	The same head; lower view	15
”	6	2	The same head; upper view	15
”	7	2	A larger larva of same, 9½ millims. long	10
”	8	2	Head of same larva; upper view	10
”	9	3	A still larger larva of same, 13½ millims. long	15
”	10	2	First of a series of transversely vertical sections of a larva of *Acipenser ruthenus*, 9½ millims. long.	40
”	11	2	Second section of same	40
”	12	2	Third section of same	40
13	1	2	Fourth section of same	40
”	2	2	Fifth section of same	40
”	3	2	Sixth section of same	40
”	4	2	Seventh section of same	40
”	5	2	Eighth section of same	40
”	6	2	Side view of dissected head of another larva, 9½ millims. long	26⅔
”	7	2	Under view of head of larva of *Acipenser ruthenus*, 9½ millims. long	14
”	8	3	Head of a larger larva of same, 13½ millims. long; under view	14
”	9	3	The same object; upper view	14
”	10	3	Horizontal section of head of a larva of the same, 13½ millims. long	40
”	11	3	Larva of *Acipenser ruthenus*, 13½ millims. long; side view of dissected head	20
”	12	3	Visceral arches of the same head; upper view	20

Plate.	Fig.	Stage.		Number of times magnified.
14	1	2	Larva of *Acipenser ruthenus*, 9½ millims. long; vertical section of head	26
„	2	3	Second horizontal section of larva of same, 13½ millims. long (see Plate 13, fig. 10).	40
„	3	3	Third section of same head	40
„	4	3	Same head; basis cranii, upper view	20
„	5	3	Same dissection as last; lower view	20
„	6	3	Maxillary and dentary of same dissection; side view.	53
„	7	3	First of a series of sections of the largest larva of *Acipenser ruthenus*, 14½ millims. long. . . .	30
„	8	3	Second section of same	30
„	9	3	Third section of same	30
„	10	3	Fourth section of same	30
„	11	3	Fifth section of same	30
„	12	3	Sixth section of same	30
„	13	3	Seventh section of same	30
„	14	3	Eighth section of same	30
15	1	3	(Same sections as on last Plate, continued.) Ninth section of same	30
„	2	3	Tenth section of same	30
„	3	3	Eleventh section of same	30
„	4	3	Twelfth section of same	30
„	5	3	Thirteenth section of same	30
„	6	3	Fourteenth section of same	30
„	7	3	Fifteenth section of same.	30
„	8	3	Sixteenth section of same	30
„	9	3	Seventeenth section of same.	30
„	10	4	Head of a young specimen of *Acipenser sturio*, 8 inches long; side view	1⅓
„	11	4	The same; upper view.	1⅓
„	12	4	The same; lower view.	1⅓

Plate.	Fig.	Stage.		Number of times magnified.
15	13	4	Dissection of head of same; upper view	2⅔
„	14	4	First of a series of sections of same	8
„	15	4	Second section of same	8
„	16	4	Third section of same	8
16	1	4	Dissection of head of a younger specimen of same, 7¼ inches long; side view	2⅔
„	2	4	A vertical section of the head of another specimen, 8 inches long	2⅔
„	3	4	Dissected skull of young *Acipenser sturio,* 8 inches long; lower view.	2⅔
„	4	4	The same; and same view, with visceral arches attached	2⅔
„	5	4	Visceral arches of same; upper view	2⅔
„	6	4	Fore part of same object; lower view	2⅔
„	7	4	Part of same as last; inner view	8
„	8	4	Fourth section of same (see Plate 15, figs. 14–16) .	4
„	9	4	Fifth section of same	4
„	10	4	Sixth section of same	4
„	11	4	Seventh section of same	4
„	12	4	Eighth section of same	4
17	1	4	(Continuation of sections of head of *Acipenser sturio,* 8 inches long.) Ninth section of same .	4
„	2	4	Tenth section of same	4
„	3	4	Eleventh section of same	4
„	4	4	Twelfth section of same	4
„	5	4	Thirteenth section of same	4
„	6	4	Fourteenth section of same	4
„	7	4	Fifteenth section of same.	4
„	8	4	Sixteenth section of same	4
„	9	4	Seventeenth section of same.	4
„	10	4	Eighteenth section of same	4
„	11	4	Nineteenth section of same	4

Plate.	Fig.	Stage.		Number of times magnified.
17	12	6	Pterygo-quadrate plate of old individual of *Acipenser sturio;* outer view . . .	$\frac{1}{3}$ natural size.
,,	13	6	The same; inner view.	$\frac{1}{3}$ natural size.
18	1	6	Right orbital region of old specimen . .	$\frac{1}{2}$ natural size.
,,	2	6	Left orbital region of old specimen . .	$\frac{1}{2}$ natural size.
,,	3	5	Basis cranii, orbital region, in adult *Acipenser sturio*	Slightly enlarged.
,,	4	5	Palato-quadrate apparatus of same, from above	Slightly enlarged.
,,	5	5	Visceral arches of same, side view. . .	$\frac{3}{4}$ natural size.
,,	6	5	The same; section of hyomandibular . .	Slightly enlarged.
,,	7	5	The same; section in another specimen .	Slightly enlarged.
,,	8	5	Mandible of same; inner view	Slightly enlarged.
,,	9	5	Section of cerato-hyal of same	Slightly enlarged.
,,	10	5	Visceral arches of same, from below . .	Slightly enlarged.

LIST OF ABBREVIATIONS.

The Roman figures indicate nerves or their foramina.

ad.m.	Adductor mandibulæ.
al.s.	Alisphenoidal region.
al.s′.	Alisphenoidal bony plate.
an.	Anus.
aq.v.	Aqueduct of vestibule.
a.s.c.	Anterior semicircular canal.
au.	Auditory capsule.
bb.	Barbel.
b.br.	Basi-branchial.
C^1.	Fore-brain.
C^2.	Mid-brain.
C^3.	Hind-brain.
d.	Dentary.
e.	Eye-ball.
e.br.	Epi-branchial.
e.eth′.	Ethmoidal bony plate.
e.o.	Ex-occipital region.
e.pa.	Ethmo-palatine.
e.pa′.	Extra ethmo-palatine.
f′.	Frontal bony scute.
fo.	Fontanelle.
g.	Gills.
h.	Heart.
h.br.	Hypo-branchial.
h.hy.	Hypo-hyal.
hm.	Hyomandibular.
hm.r.	Hyomandibular ray.
h.s.c.	Horizontal semicircular canal.
hy.	Hyoid fold.
i.hy.	Inter-hyal.
i.tr.	Intertrabecula.
iv.	Investing mass.
l.	Liver.
l.l.	Lower lip.
m.	Mouth.
mk.	MECKEL's cartilage.
mn.	Mandibular fold or bar.
ms.pg.	Mesopterygoid.
mt.pg′.	Median metapterygoids.
mt.pg″.	Lateral metapterygoids.
mx.	Maxillary.
nc.	Notochord.
ol.	Olfactory capsule.
op.	Opercular scute.
o.s′.	Orbito-sphenoidal bony plate.
p′.	Parietal scute.
pa.	Palatine bone.
pa.s.	Parasphenoid.
p.br.	Pharyngo-branchial.
p.f.	Pectoral fin.
pg.	Pterygoid bone.
pg.q.	Pterygo-quadrate.
px.	Pharynx.
p.op.	Pre-opercular bone.
pr.o′.	Prootic bony plate.
p.s.c.	Posterior semicircular canal.
pt.hm.	Protractor hyomandibularis.
pt.ob.	Postorbital scute.
q.c.	Quadrate condyle.
s.ob.	Superorbital scute.
sp.	Spiracle.
sp.r.	Spiracular ray.
sq′.	Squamosal scute.
su.ob.	Suborbital scute.
sy.	Symplectic.
t.	Teeth.
t.cr.	Tegmen cranii.
tr.	Trabeculæ.
u.l.	Upper lip.
v.	Vomerine bone.
vb.	Vestibule.
v.f.	Ventral fin.
y.st.	Yolk-mass and stomach.

PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY.

Note to be inserted at p. 187, Part I. 1882.

CORRECTION TO A PAPER ENTITLED

" On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains."

Since this paper has left my hands I have discovered an error in the work. The error does not affect the physical conclusions, except in one unimportant respect; had it done so I should probably have found it out long ago.

Throughout the paper the normal stresses P, Q, R *require an additional term* W_t. *The only function of these stresses used for obtaining physical results is* P − R, *and it remains unchanged when the correction is made.* § 10 *must however be erased.*

The error takes its origin in § (1). Thomson's solution (1), when reduced to the form applicable to the incompressible solid, is the solution of the equations $-\frac{dp}{dx} + \upsilon\nabla\alpha^2 = \frac{dW}{dx}$, and two others. The solution required is that of $-\frac{dp}{dx} + \upsilon\nabla\alpha^2 = 0$, and two others. The W involved in my solution is not the potential of a true bodily force, but only an "effective potential" producing the same strains as those due to the weight of the continents and mountains, but causing a different hydrostatic pressure. When therefore p is determined from Thomson's solution, that p is really equal to $p + W_t$ of the problem of the continents. Hence equation (3) should be $p = -\left(1 + \frac{i}{I}\right)W_t$, instead of $p = -\frac{i}{I}W_t$. The correction to (3) must be carried on through the rest of the paper, and obviously it merely adds W_t to the stresses P, Q, R, leaving P − R unchanged.

The error would have been avoided had I, as suggested on p. 190, worked directly from the equations of equilibrium of the elastic incompressible solid, instead of from Thomson's solution.

When the solid is compressible, this method of "effective potential" [see "The Tides of a Viscous Spheroid," *Phil. Trans.* Part I. 1879, pp. 7—9] for including all the effects of gravitation, is not applicable without certain additional terms in α, β, γ. Hence § 10 is erroneous, inasmuch as the expressions for the strains and stresses are incomplete. The correction of § 10 (which is not difficult) would require too much space to be carried out in this note.

G. H. DARWIN.

Aug. 1, 1882.

IV. *On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains.*

By G. H. DARWIN, *F.R.S.*

Received June 11,—Read June 16, 1881.

[PLATES 19, 20.]

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Part II. SUMMARY AND DISCUSSION 218

IN this paper I have considered the subject of the solidity and strength of the materials of which the earth is formed, from a point of view from which it does not seem to have been hitherto discussed.

The first part of the paper is entirely devoted to a mathematical investigation, based upon a well-known paper of Sir WILLIAM THOMSON'S. The second part consists of a summary and discussion of the preceding work. In this I have tried, as far as possible, to avoid mathematics, and I hope that a considerable part of it may prove intelligible to the non-mathematical reader.

I.

THE MATHEMATICAL INVESTIGATION.

§ 1. *On the state of internal stress of a strained elastic sphere.*

Let there be a homogeneous elastic sphere, for which $\omega-\frac{1}{3}v$ is the modulus of compressibility (or incompressibility, as I shall call it) and v the rigidity.* Take the centre of the sphere as the origin for a set of rectangular axes x, y, z. Let the sphere be subjected to no surface stresses, let it be devoid of gravitation, but subject to internal force such that the force acting on a unit volume of the elastic solid is expressible by a gravitation potential W_i, a solid spherical harmonic of the i^{th} degree of the coordinates x, y, z.

Let w be the density of the elastic solid, a the radius of the sphere, and r the radius vector of any point measured from the centre of the sphere.

Sir WILLIAM THOMSON has investigated the state of internal strain produced under the conditions above described. If α, β, γ be the displacements his solution is as follows :—

$$\left.\begin{array}{l}\alpha=(E_i a^2-F_i r^2)\dfrac{dW_i}{dx}-G_i r^{2i+3}\dfrac{d}{dx}(W_i r^{-2i-1}) \\ \text{where} \\ E_i=\dfrac{i[(i+2)\omega-v]}{2(i-1)v\{[2(i+1)^2+1]\omega-(2i+1)v\}} \\ F_i=\dfrac{(i+1)(2i+3)\omega-(2i+1)v}{2(2i+1)v\{[2(i+1)^2+1]\omega-(2i+1)v\}} \\ G_i=\dfrac{i\omega}{(2i+1)v\{[2(i+1)^2+1]\omega-(2i+1)v\}}\end{array}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (1)$$

and similar expressions for β and γ.†

Now let P, Q, R, S, T, U be the six stresses, across three planes mutually at right angles at the point x, y, z, estimated as is usual in works on the theory of elasticity.

Let P, Q, R be tractions and not pressures, and let p be the hydrostatic pressure at the point x, y, z.

Then P+Q+R being an invariant of the stress quadric, we have,

$$p=-\tfrac{1}{3}(\mathrm{P}+\mathrm{Q}+\mathrm{R})$$

if $\delta=\dfrac{d\alpha}{dx}+\dfrac{d\beta}{dy}+\dfrac{d\gamma}{dz}$, so that δ is the dilatation, then according to the usual formulas,‡

---

* The phraseology adopted by THOMSON and TAIT (first edition) and others seems a little unfortunate. One might be inclined to suppose that compressibility and rigidity were things of the same nature; but rigidity and the reciprocal of compressibility are of the same kind. If one may give exact meanings to old words of somewhat general meanings, then one may pair together compressibility and "pliancy," and call the moduli for the two sorts of elasticity the "incompressibility" and rigidity.

† THOMSON and TAIT'S 'Nat. Phil.,' § 834, (8) and (9); or Phil. Trans., 1863, p. 573.

‡ THOMSON and TAIT'S 'Nat. Phil.,' § 693.

$$P=(\omega-v)\delta+2v\frac{d\alpha}{dx}$$

$$T=v\left(\frac{d\alpha}{dz}+\frac{d\gamma}{dx}\right)$$

and the other four stresses are expressed from these by cyclic changes of P, Q, R; S, T, U; α, β, γ; x, y, z.

The first task is to find p.

Now by adding P, Q, R together we have,

$$p=-(\omega-\tfrac{1}{3}v)\delta$$

We must now find δ from (1).

By differentiation

$$\frac{d\alpha}{dx}=(E_ia^2-F_ir^2)\frac{d^2W_i}{dx^2}-2F_ix\frac{dW_i}{dx}-G_ir^{2i+3}\frac{d^3}{dx^3}(W_ir^{-2i-1})-(2i+3)G_ir^{2i+1}x\frac{d}{dx}(W_ir^{-2i-1})$$

and similar expressions for $d\beta/dy$, $d\gamma/dz$.

Now W_i, W_ir^{-2i-1} are spherical harmonics of degrees i, $-i-1$, and are also homogeneous functions of the same degrees. If therefore we add the three expressions together, and note the properties of harmonics and of homogeneous functions, we have

$$\delta=-2iF_iW_i+(2i+3)(i+1)G_iW_i$$

Omitting for brevity that part of the divisors in the expressions for F and G which is common to both,

$$\begin{aligned}-2iF_i+(2i+3)(i+1)G_i&=-i(i+1)(2i+3)\omega+i(2i+1)v+(2i+3)(i+1)i\omega\\&=\ i(2i+1)v\end{aligned}$$

and we have, on introducing the omitted denominator,

$$\delta=\frac{i}{[2(i+1)^2+1]\omega-(2i+1)v}W_i$$

And

$$p=\frac{-i(\omega-\frac{1}{3}v)}{[2(i+1)^2+1]\omega-(2i+1)v}W_i$$

Throughout the rest of this paper (excepting in § 10) the elastic sphere will be treated as incompressible, so that ω is to be considered as infinitely large compared with v.

Henceforth I write

$$I=2(i+1)^2+1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and when ω is infinite compared with v, we have,

$$p=-\frac{i}{I}W_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Also we may put

$$\left.\begin{aligned}\mathrm{P}&=-p+2v\frac{d\alpha}{dx}\\ \mathrm{T}&=v\left(\frac{d\alpha}{dz}+\frac{d\gamma}{dx}\right)\end{aligned}\right\} \quad \dots\dots\dots \quad (4)$$

And on putting ω infinite in (1) we have

$$\left.\begin{array}{l}\alpha=\dfrac{1}{Iv}\left[\left\{\dfrac{i(i+2)}{2(i-1)}a^2-\dfrac{(i+1)(2i+3)}{2(2i+1)}r^2\right\}\dfrac{dW_i}{dx}-\dfrac{i}{2i+1}r^{2i+3}\dfrac{d}{dx}(W_ir^{-2i-1})\right]\\ \text{and symmetrical expressions for } \beta \text{ and } \gamma.\end{array}\right\} \quad \dots \quad (5)$$

The hydrostatic pressure might have been found from this general solution for the case of incompressibility, but in order to do so it would have been necessary to go back to the equations of equilibrium of the solid, and I prefer to deduce it from Sir WILLIAM THOMSON'S solution in the more general case.

Since

$$r^{2i+3}\frac{d}{dx}(W_ir^{-2i-1})=-(2i+1)xW_i+r^2\frac{dW_i}{dx}$$

and since

$$(i+1)(2i+3)+2i=(2i+1)(i+3)$$

we may write α as follows:—

$$\alpha=\frac{1}{2Iv}\left[\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{dW_i}{dx}+2ixW_i\right] \quad \dots\dots \quad (6)$$

In order to find the stresses P, Q, &c., we must now evaluate $\frac{d\alpha}{dx}$, $\frac{d\alpha}{dz}$, $\frac{d\gamma}{dx}$, &c. Differentiating (6) with regard to x,—

$$2Iv\frac{d\alpha}{dx}=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dx^2}-6x\frac{dW_i}{dx}+2iW_i \quad \dots\dots \quad (7)$$

Differentiating with regard to z,—

$$2Iv\frac{d\alpha}{dz}=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dxdz}+2\left\{ix\frac{dW_i}{dz}-(i+3)z\frac{dW_i}{dx}\right\} \quad \dots \quad (8)$$

and by symmetry

$$2Iv\frac{d\gamma}{dx}=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dxdz}+2\left\{iz\frac{dW_i}{dx}-(i+3)x\frac{dW_i}{dz}\right\} \quad \dots \quad (9)$$

Adding (8) and (9) together and dividing by 2, we have

$$Iv\left(\frac{d\alpha}{dz}+\frac{d\gamma}{dx}\right)=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dxdz}-3\left\{x\frac{dW_i}{dz}+z\frac{dW_i}{dx}\right\} \quad \dots \quad (10)$$

Hence from (3) (4) (7) and (10) we have,

$$\left.\begin{aligned} IP&=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dx^2}-6x\frac{dW_i}{dx}+3iW_i \\ IT&=\left\{\frac{i(i+2)}{i-1}a^2-(i+3)r^2\right\}\frac{d^2W_i}{dxdz}-3\left(x\frac{dW_i}{dz}+z\frac{dW_i}{dx}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (14)$$

where $I=2(i+1)^2+1$.

The expressions for Q, R, S, U may be written down from these by means of cyclic changes of the symbols.

These are the required expressions for the stresses at any point in the interior of the sphere.

In order to find the magnitude and direction of the principal stress-axes at any point it would be necessary to solve a cubic equation. The solution of this equation appears to be difficult, but the special case in which it reduces to a quadratic equation will fortunately give adequate results. It may be seen from considerations of symmetry that if W_i be a zonal harmonic, two of the principal stress-axes lie in a meridional plane and the third is perpendicular thereto. Moreover the greatest and least stress-axes are those which lie in that plane, and the mean stress-axis is that which is perpendicular thereto. If this is not obvious to the reader at present, it will become so later.

I shall therefore take W_i to be a zonal harmonic, and as the future developments will be by means of series (which though finite will be long for the higher orders of harmonics) I shall attend more especially to the equatorial regions of the sphere.

§ 2. *The determination of the stresses when the disturbing potential is an even zonal harmonic.*

If θ be colatitude the expression for a zonal surface harmonic or LEGENDRE'S function of order i is

$$\cos^i\theta-\frac{i(i-1)}{4.(1\,!)^2}\cos^{i-2}\theta\sin^2\theta+\frac{i(i-1)(i-2)(i-3)}{4^2(2\,!)^2}\cos^{i-4}\theta\sin^4\theta-\ldots$$

or if we begin by the other end of the series, and take i as an even number, the expression is

$$(-)^{\frac{1}{2}i}\frac{i\,!}{2^i\{\frac{1}{2}i\,!\}^2}\left[\sin^i\theta-\frac{i^2}{2\,!}\sin^{i-2}\theta\cos^2\theta+\frac{i^2(i-2)^2}{4\,!}\sin^{i-4}\theta\cos^4\theta-\ldots\right]$$

This latter is the appropriate form when we wish to consider especially the equatorial regions, because $\cos\theta$ is small for that part of the sphere.

There is of course a similar formula when i is odd, but of this I shall make no use.

Now let $\rho^2=y^2+z^2$, so that $\sin\theta=\rho/r$, $\cos\theta=z/r$.

Then we may put

$$\mathrm{W}_i=\rho^i-\frac{i^2}{2!}\rho^{i-2}z^2+\frac{i^2(i-2)^2}{4!}\rho^{i-4}z^4-\frac{i^2(i-2)^2(i-4)^2}{6!}\rho^{i-6}z^6+\ldots\ldots \quad (12)$$

W_i is a solid zonal harmonic of degree i; but $r^{-i}W_i$ requires multiplication by a factor $(-)^{\frac{1}{2}i}i!/2^i\{\frac{1}{2}i!\}^2$ in order to make it a LEGENDRE'S function.

The factors by which W_i must be deemed to be multiplied in order that it may be a potential, will be dropped for the present, to be inserted later. Or we may, if we like, suppose that the units of length or of time are so chosen as to make the factor equal to unity.

Now let

$$\beta_0=1,\ \beta_2=\frac{i^2}{2!},\ \beta_4=\frac{i^2(i-2)^2}{4!},\ \beta_6=\frac{i^2(i-2)^2(i-4)^2}{6!}\ \&c. \ldots\ldots \quad (13)$$

Then, dropping the suffix to W for brevity, we may write

$$W=\beta_0\rho^i-\beta_2\rho^{i-2}z^2+\beta_4\rho^{i-4}z^4-\beta_6\rho^{i-6}z^6+\ldots\ldots\ldots \quad (14)$$

I shall now find P, Q, R, T at any point in the meridional plane which is determined by $y=0$.

In evaluating the first differential coefficients of W we must not put $y=0$, in as far as these coefficients are a first step towards the determination of the second differential coefficients. But in as far as these first coefficients are directly involved in the expressions for P, Q, R, and T, and in the second coefficients in the same expressions, we may put $y=0$, and thus write x in place of ρ.

$$\rho\frac{d\rho}{dx}=x,\ \rho\frac{d\rho}{dy}=y,\ \frac{d\rho}{dz}=0, \text{ since } \rho^2=x^2+y^2.$$

Then

$$\begin{aligned}\frac{dW}{dx}&=x[i\beta_0\rho^{i-2}-(i-2)\beta_2\rho^{i-4}z^2+(i-4)\beta_4\rho^{i-6}z^4-\ldots]\\ \frac{dW}{dy}&=y[\text{same series}\qquad\qquad\qquad\qquad\qquad]\\ \frac{dW}{dz}&=z[\qquad -2\beta_2\rho^{i-2}+4\beta_4\rho^{i-4}z^2-6\beta_6\rho^{i-6}z^4+\ldots]\end{aligned}$$

In differentiating a second time we may treat ρ as identical with x, because y is to be put equal to zero. Thus

$$\left.\begin{aligned}\frac{d^2W}{dx^2}&=i(i-1)\beta_0x^{i-2}-(i-2)(i-3)\beta_2x^{i-4}z^2+(i-4)(i-5)\beta_4x^{i-6}z^4-\ldots\\ \frac{d^2W}{dy^2}&=i\beta_0x^{i-2}-(i-2)\beta_2x^{i-4}z^2+(i-4)\beta_4x^{i-6}z^4-\ldots\\ \frac{d^2W}{dz^2}&=-1.2\beta_2x^{i-2}+3.4\beta_4x^{i-4}z^2-5.6\beta_6x^{i-6}z^4+\ldots\end{aligned}\right\}. \quad (15)$$

$$\left.\begin{aligned}&\frac{d^2W}{dxdz}=xz[-2(i-2)\beta_2x^{i-4}+4(i-4)\beta_4x^{i-6}z^2-6(i-6)\beta_6x^{i-8}z^4+\ldots]\\&\frac{d^2W}{dxdy}=0,\qquad\frac{d^2W}{dydz}=0\end{aligned}\right\}\quad.\quad(16)$$

Also treating ρ as identical with x, and putting $y=0$,

$$\left.\begin{aligned}&x\frac{dW}{dx}=i\beta_0x^i-(i-2)\beta_2x^{i-2}z^2+(i-4)\beta_4x^{i-4}z^4-\ldots\\&y\frac{dW}{dy}=0\\&z\frac{dW}{dz}=-2\beta_2x^{i-2}z^2+4\beta_4x^{i-4}z^4-6\beta_6x^{i-6}z^6+\ldots\end{aligned}\right\}\quad.\;.\;.\;.\;.\quad(17)$$

$$\left.\begin{aligned}&\left(z\frac{dW}{dx}+x\frac{dW}{dz}\right)=xz\{(i\beta_0-2\beta_2)x^{i-2}-[(i-2)\beta_2-4\beta_4]x^{i-4}z^2\\&\qquad\qquad\qquad\qquad\qquad\qquad+[(i-4)\beta_4-6\beta_6]x^{i-6}z^4-\ldots\}\\&\left(y\frac{dW}{dx}+x\frac{dW}{dy}\right)=0,\qquad\left(y\frac{dW}{dz}+z\frac{dW}{dy}\right)=0\end{aligned}\right\}\quad.\quad(18)$$

These various results have now to be introduced into the expressions (11) for P, Q, R, S, T, U.

In performing these operations it will be convenient to write J for $i(i+2)/(i-1)$. Also $r^2=\rho^2+z^2=x^2+z^2$, when $y=0$.

From these formulas we see that $S=0$, $U=0$; which shows that a meridional plane is one of the three principal planes, a result already observed from principles of symmetry.

Now

$$\left.\begin{aligned}&r^2\frac{d^2W}{dx^2}=i(i-1)\beta_0x^i+[i(i-1)\beta_0-(i-2)(i-3)\beta_2]x^{i-2}z^2\\&\qquad\qquad-[(i-2)(i-3)\beta_2-(i-4)(i-5)\beta_4]x^{i-4}z^4+\ldots\\&r^2\frac{d^2W}{dy^2}=i\beta_0x^i+[i\beta_0-(i-2)\beta_2]x^{i-2}z^2-[(i-2)\beta_2-(i-4)\beta_4]x^{i-4}z^4+\ldots\\&r^2\frac{d^2W}{dz^2}=-1.2\beta_2x^i-[1.2\beta_2-3.4\beta_4]x^{i-2}z^2+[3.4\beta_4-5.6\beta_6]x^{i-4}z^4-\ldots\end{aligned}\right\}\quad(19)$$

$$\left.\begin{aligned}&-2x\frac{dW}{dx}+iW=-i\beta_0x^i+(i-4)\beta_2x^{i-2}z^2-(i-8)\beta_4x^{i-4}z^4+\ldots\\&-2y\frac{dW}{dy}+iW=i\beta_0x^i-i\beta_2x^{i-2}z^2+i\beta_4x^{i-4}z^4-\ldots\\&-2z\frac{dW}{dz}+iW=i\beta_0x^i-(i-4)\beta_2x^{i-2}z^2+(i-8)\beta_4x^{i-4}z^4-\ldots\end{aligned}\right\}\quad.\;.\quad(20)$$

$$
\begin{aligned}
r^2\frac{d^2W}{dxdz}=xz\{&-2(i-2)\beta_2x^{i-2}-[2(i-2)\beta_2-4(i-4)\beta_4]x^{i-4}z^2\\
&+[4(i-4)\beta_4-6(i-6)\beta_6]x^{i-6}z^6-\ldots\}
\end{aligned}
\quad . \quad . \quad (21)
$$

Then multiplying (19) by $-(i+3)$, (20) by 3, and (15) by Ja^2, and adding them each to each, we get the expressions for P, Q, R.

Also multiplying (21) by $-(i+3)$, (18) by -3, and (16) by Ja^2 and adding, we get the expression for T. The results are

$$
\begin{aligned}
IP=&-[(i+3)i(i-1)+3i]\beta_0x^i\\
&\qquad+[\{(i+3)(i-2)(i-3)+3(i-4)\}\beta_2-(i+3)i(i-1)\beta_0]x^{i-2}z^2\\
&-[\{(i+3)(i-4)(i-5)+3(i-8)\}\beta_4-(i+3)(i-2)(i-3)\beta_2]x^{i-4}z^4\\
&+[\{i+3)(i-6)(i-7)+3(i-12)\}\beta_6-(i+3)(i-4)(i-5)\beta_4]x^{i-6}z^6-\ldots\\
&+Ja^2[i(i-1)\beta_0x^{i-2}-(i-2)(i-3)\beta_2x^{i-4}z^2+(i-4)(i-5)\beta_4x^{i-6}z^4-\ldots]
\end{aligned}
$$

$$
\begin{aligned}
IR=&[(i+3).1.2\beta_2+3i\beta_0]x^i-[(i+3).3.4\beta_4-\{(i+3).1.2-3(i-4)\}\beta_2]x^{i-2}z^2\\
&+[(i+3).5.6\beta_6-\{(i+3).3.4-3(i-8)\}\beta_4]x^{i-4}z^4\\
&-[(i+3).7.8\beta_8-\{(i+3).5.6-3(i-12)\beta_6]x^{i-6}z^6+\ldots\\
&-Ja^2[1.2\beta_2x^{i-2}-3.4\beta_4x^{i-4}z^2+5.6\beta_6x^{i-6}z^4-\ldots]
\end{aligned}
$$

$$
\begin{aligned}
IQ=&-[(i+3)i-3i]\beta_0x^i+[\{(i+3)(i-2)-3i\}\beta_2-(i+3)i\beta_0]x^{i-2}z^2\\
&-[\{(i+3)(i-4)-3i\}\beta_4-(i+3)(i-2)\beta_2]x^{i-4}z^4\\
&+[\{(i+3)(i-6)-3i\}\beta_6-(i+3)(i-4)\beta_4]x^{i-6}z^6-\ldots\\
&+Ja^2[ix^{i-2}-(i-2)\beta_2x^{i-4}z^2+(i-4)\beta_4x^{i-6}z^4-\ldots]
\end{aligned}
$$

$$
\begin{aligned}
\frac{IT}{xz}=&[\{(i+3)2(i-2)+3.2\}\beta_2-3i\beta_0]x^{i-2}\\
&-[\{(i+3)4(i-4)+3.4\}\beta_4-\{(i+3)2(i-2)+3(i-2)\}\beta_2]x^{i-4}z^2\\
&+[\{(i+3)6(i-6)+3.6\}\beta_6-\{(i+3)4(i-4)+3(i-4)\}\beta_4]x^{i-6}z^4\\
&-[\{(i+3)8(i-8)+3.8\}\beta_8-\{(i+3)6(i-6)+3(i-6)\}\beta_6]x^{i-6}z^6+\ldots\\
&-Ja^2[2(i-2)\beta_2x^{i-4}-4(i-4)\beta_4x^{i-6}z^2+6(i-6)\beta_6x^{i-8}z^4-\ldots]
\end{aligned}
$$

The general law of formation of the successive coefficients is obvious, and it is easy to write down the general term in each of the eight series involved in these four expressions; the best way indeed of obtaining the formulas given below is to write down and transform the general term.

The semi-polar coordinates used hitherto are not so convenient as true polar coordinates; I therefore substitute r, radius vector, and l, latitude, for the x, z system, and putting $x=r\cos l$, $z=r\sin l$ write

$$\left.\begin{aligned}
\mathrm{P}&=r^i\cos^i l(A_0+A_2\tan^2 l+A_4\tan^4 l+\ldots)\\
&\qquad+a^2r^{i-2}\cos^{i-2}l(B_0+B_2\tan^2 l+B_4\tan^4 l+\ldots)\\
\mathrm{R}&=r^i\cos^i l(C_0+C_2\tan^2 l+C_4\tan^4 l+\ldots)\\
&\qquad+a^2r^{i-2}\cos^{i-2}l(D_0+D_2\tan^2 l+D_4\tan^4 l+\ldots)\\
\mathrm{T}&=r^i\sin l\cos^{i-1}l(E_0+E_2\tan^2 l+E_4\tan^4 l+\ldots)\\
&\qquad+a^2r^{i-2}\sin l\cos^{i-3}l(F_0+F_2\tan^2 l+F_4\tan^4 l+\ldots)\\
\mathrm{Q}&=r^i\cos^i l(G_0+G_2\tan^2 l+G_4\tan^4 l+\ldots)\\
&\qquad+a^2r^{i-2}\cos^{i-2}l(H_0+H_2\tan^2 l+H_4\tan^4 l+\ldots)
\end{aligned}\right\}\quad(22)$$

Then introducing for J and for the β's their values in terms of i, I find that the coefficients A, B, &c., are reducible to the forms given in the following equations :—

$$\left.\begin{aligned}
IA_0&=-\frac{1}{0!}\{i(i+2)(i-0)-3.0.(i+1)\}+0.(i+3)(i+2)(i+1)=-i^2(i+2)\\
IA_2&=\frac{i^2}{2!}\{i(i-0)(i-2)-3.2(i-1)\}-\frac{1}{0!}(i+3)(i+0)(i-1)\\
IA_4&=-\frac{i^2(i-2)^2}{4!}\{i(i-2)(i-4)-3.4(i-3)\}+\frac{i^2}{2!}(i+3)(i-2)(i-3)\\
IA_6&=\frac{i^2(i-2)^2(i-6)^2}{6!}\{i(i-4)(i-6)-3.6(i-5)\}-\frac{i^2(i-2)^2}{4!}(i+3)(i-4)(i-5)\\
\&\text{c.}&=\&\text{c.}\\
IB_0&=\frac{i(i+2)}{i-1}\,\frac{1}{0!}i(i-1)\\
IB_2&=-\frac{i(i+2)}{i-1}\,\frac{i^2}{2!}(i-2)(i-3)\\
IB_4&=\frac{i(i+2)}{i-1}\,\frac{i^2(i-2)^2}{4!}(i-4)(i-5)\\
\&\text{c.}&=\&\text{c.}
\end{aligned}\right\}\quad(23)$$

$$\left.\begin{aligned}
IC_0&=\frac{1}{0!}\{(i+3)i^2-[i(1(-2)-1)+3.0.1]\}=i[(i+1)(i+2)+1]\\
IC_2&=-\frac{i^2}{2!}\{(i+3)(i-2)^2-[i(3.0-1)+3.2.3]\}\\
IC_4&=\frac{i^2(i-2)^2}{4!}\{(i+3)(i-4)^2-[i(5.2-1)+3.4.5]\}\\
IC_6&=-\frac{i^2(i-2)^2(i-4)^2}{6!}\{(i+3)(i-6)^2-[i(7.4-1)+3.6.7]\}\\
\&\text{c.}&=\&\text{c.}\\
ID_0&=-\frac{i(i+2)}{i-1}\,\frac{i^2}{0!}\\
ID_2&=\frac{i(i+2)}{i-1}\,\frac{i^2(i-2)^2}{2!}\\
ID_4&=-\frac{i(i+2)}{i-1}\,\frac{i^2(i-2)^2(i-4)^2}{4!}\\
\&\text{c.}&=\&\text{c.}
\end{aligned}\right\}\quad(24)$$

$$\left.\begin{aligned}
IE_0&=-\frac{1}{1!}i[1(3i+3)-i(i-0)(i+1)]=i(i+1)(i^2\ \ 3)\\
IE_2&=\frac{i^2}{3!}(i-2)[3(5i+3)-i(i-2)(i-1)]\\
IE_4&=-\frac{i^2(i-2)^2}{5!}(i-4)[5(7i+3)-i(i-4)(i-3)]\\
IE_6&=\frac{i^2(i-2)^2(i-4)^2}{7!}(i-6)[7(9i+3)-i(i-6)(i-5)]\\
\&c.&=\&c.\\
IF_0&=-\frac{i^3(i^2-4)}{i-1}\\
IF_2&=\frac{i^3(i^2-4)}{i-1}\,\frac{(i-2)(i-4)}{3!}\\
IF_4&=-\frac{i^3(i^2-4)}{i-1}\,\frac{(i-2)(i-4)^2(i-6)}{5!}\\
IF_6&=\frac{i^3(i^2-4)}{i-1}\,\frac{(i-2)(i-4)^2(i-6)^2(i-8)}{7!}\\
\&c.&=\&c.
\end{aligned}\right\}\quad . \ . \ . \ (25)$$

$$\left.\begin{aligned}
IG_0&=-\frac{1}{0!}\{i(i-0)-3.0\}+0.(i+3)(i+2)=-i^2\\
IG_2&=\frac{i^2}{2!}\{i(i-2)-3.2\}-\frac{1}{0!}(i+3)i\\
IG_4&=-\frac{i^2(i-2)^2}{4!}\{i(i-4)-3.4\}+\frac{i^2}{2!}(i+3)(i-2)\\
\&c.&=\&c.\\
IH_0&=\frac{i(i+2)}{i-1}\,\frac{1}{0!}i\\
IH_2&=-\frac{i(i+2)}{i-1}\,\frac{i^2}{2!}(i-2)\\
IH_4&=\frac{i(i+2)}{i-1}\,\frac{i^2(i-2)^2}{4!}(i-4)\\
\&c.&=\&c.
\end{aligned}\right\}\quad . \ . \ (26)$$

These sets of coefficients are all written down in such a form that the laws of their formation are obvious, and the general terms may easily be found. I have computed their values from these formulas for the even zonal harmonics of orders 2, 4, 6, 8, 10, 12; the results are given in the following tables both in the form of fractions and of decimals approximately equal to those fractions.

The G's and H's were not computed because their values were not required for subsequent operations.

TABLE I.—The coefficients for expressing the stress P.

i	A_0	A_2	A_4	A_6	B_0	B_2	B_4	B_6
2	$-\frac{16}{19}$ −·8421	$-\frac{22}{19}$ −1·1579			$+\frac{16}{19}$ +·8421			
4	$-\frac{32}{17}$ −1·8824	$+\frac{28}{51}$ +·5490	$+\frac{48}{17}$ +2·8235		$+\frac{32}{17}$ +1·8824	$-\frac{128}{51}$ −2·5098		
6	$-\frac{32}{11}$ −2·9091	+18 +18·0000	$+\frac{184}{11}$ +16·7273	$-\frac{272}{55}$ −4·9455	$+\frac{32}{11}$ +2·9091	$-\frac{1152}{55}$ −20·9455	$+\frac{256}{55}$ +4·6545	
8	$-\frac{640}{163}$ −3·9264	$+\frac{10328}{163}$ +63·3620	$-\frac{2112}{163}$ −12·9571	$-\frac{12160}{163}$ −74·6012	$+\frac{640}{163}$ +3·9264	$-\frac{76800}{1141}$ −67·3094	$+\frac{92160}{1141}$ +80·7713	$-\frac{8192}{1141}$ −7·1797
10	$-\frac{400}{81}$ −4·9383	$+\frac{36130}{243}$ +148·6831	$-\frac{69200}{243}$ −284·7737	$-\frac{56000}{243}$ −230·4527	$+\frac{400}{81}$ +4·9383	$-\frac{112000}{729}$ −153·6352	$+\frac{320000}{729}$ +438·9561	$-\frac{51200}{243}$ −210·6996
12	$-\frac{672}{113}$ −5·9469	$+\frac{32316}{113}$ +285·9823	$-\frac{138000}{113}$ −1221·2389	$+\frac{24000}{113}$ +212·3894	$+\frac{672}{113}$ +5·9469	$-\frac{362880}{1243}$ −291·9389	$+\frac{1881600}{1243}$ +1513·7570	$-\frac{2150400}{1243}$ −1730·0080

TABLE II.—The coefficients for expressing the stress R.

i	C_0	C_2	C_4	C_6	D_0	D_2	D_4	D_6
2	$+\frac{26}{19}$ +1·3684	$+\frac{32}{19}$ +1·6842			$-\frac{32}{19}$ −1·6842			
4	$+\frac{124}{51}$ +2·4314	$-\frac{112}{51}$ −2·1961	$-\frac{256}{51}$ −5·0196		$-\frac{128}{51}$ −2·5098	$+\frac{256}{51}$ +5·0196		
6	$+\frac{38}{11}$ +3·4545	−24 −24·0000	$-\frac{208}{11}$ −18·9091	$+\frac{512}{55}$ +9·3091	$-\frac{192}{55}$ −3·4909	$+\frac{1536}{55}$ +27·9273	$-\frac{512}{55}$ −9·3091	
8	$+\frac{728}{163}$ +4·4663	$-\frac{12352}{163}$ −75·7791	$+\frac{4224}{163}$ +25·9141	$+\frac{76288}{815}$ +93·6049	$-\frac{5120}{1141}$ −4·4873	$+\frac{92160}{1141}$ +80·7713	$-\frac{122880}{1141}$ −107·6950	$+\frac{16384}{1141}$ +14·3593
10	$+\frac{1330}{243}$ +5·4733	$-\frac{41200}{243}$ −169·5473	$+\frac{84800}{243}$ +348·9712	$+\frac{60160}{243}$ +247·5720	$-\frac{4000}{729}$ −5·4870	$+\frac{128000}{729}$ +175·5830	$-\frac{384000}{729}$ −526·7490	$+\frac{204800}{729}$ +280·9328
12	$+\frac{732}{113}$ +6·4779	$-\frac{35856}{113}$ −317·3097	$+\frac{158400}{113}$ +1401·7700	$-\frac{38400}{113}$ −339·8230	$-\frac{8064}{1243}$ −6·4875	$+\frac{403200}{1243}$ +324·3765	$-\frac{2150400}{1243}$ −1730·0080	$+\frac{2580480}{1243}$ +2076·0097

TABLE III.—The coefficients for expressing the stress T.

i	E_0	E_2	E_4	E_6	F_0	F_2	F_4	F_6
2	$+\frac{6}{19}$ $+\cdot 3158$							
4	$+\frac{260}{51}$ $+5\cdot 0980$	$+\frac{80}{17}$ $+4\cdot 7059$			$-\frac{256}{51}$ $-5\cdot 0196$			
6	$+14$ $+14\cdot 0000$	$-\frac{56}{11}$ $-5\cdot 0909$	$-\frac{1008}{55}$ $-18\cdot 3273$		$-\frac{768}{55}$ $-13\cdot 9636$	$+\frac{1024}{55}$ $+18\cdot 6182$		
8	$+\frac{4392}{163}$ $+26\cdot 9448$	$-\frac{13248}{163}$ $-81\cdot 2761$	$-\frac{10368}{163}$ $-63\cdot 6074$	$+\frac{244224}{5705}$ $+42\cdot 8088$	$-\frac{30720}{1141}$ $-26\cdot 9238$	$+\frac{122880}{1141}$ $+107\cdot 6950$	$-\frac{49152}{1141}$ $-43\cdot 0780$	
10	$+\frac{10670}{243}$ $+43\cdot 9095$	$-\frac{74800}{243}$ $-307\cdot 8189$	$+\frac{17600}{243}$ $+72\cdot 4280$	$+\frac{577280}{1701}$ $+339\cdot 3769$	$-\frac{32000}{729}$ $-43\cdot 8958$	$+\frac{256000}{729}$ $+351\cdot 1660$	$-\frac{102400}{243}$ $-421\cdot 3992$	$+\frac{409600}{5103}$ $+80\cdot 2664$
12	$+\frac{7332}{113}$ $+64\cdot 8850$	$-\frac{90480}{113}$ $-800\cdot 7089$	$+\frac{137280}{113}$ $+1214\cdot 8673$	$+\frac{99840}{113}$ $+883\cdot 5398$	$-\frac{80640}{1243}$ $-64\cdot 8753$	$+\frac{1075200}{1243}$ $+865\cdot 0040$	$-\frac{2580480}{1243}$ $-2076\cdot 0097$	$+\frac{1474560}{1243}$ $+1186\cdot 2912$

If W be a 2nd, 4th, or 6th harmonic these tables give the complete expressions for P, R, and T; if W be an 8th harmonic the only further coefficients required are A_8 and C_8.

For the cases of the 10th and 12th harmonics the values in the tables are sufficient to give the stresses approximately over a wide equatorial belt, because the series for P, R, T proceed by powers of the tangent of the latitude, and the omitted terms involve high powers of that tangent. It would hardly be safe however to apply the formula—at least as regards the 12th harmonic—for latitudes greater than 15°, because the coefficients are large.

§ 3. *On the direction and magnitude of the principal stresses in a strained elastic solid.*

Let P, Q, R, S, T, U specify the stresses in a homogeneously stressed and straine elastic solid. Let l, m, n be the direction cosines of a principal stress axis.

The consideration, that at the extremity of a principal axis the normal to the stre quadric is coincident with the radius vector, gives the equations

$$\begin{gathered}(\mathrm{P}-\lambda)l+\mathrm{U}m+\mathrm{T}n=0\\ \mathrm{U}l+(\mathrm{Q}-\lambda)m+\mathrm{S}n=0\\ \mathrm{T}l+\mathrm{S}m+(\mathrm{R}-\lambda)n=0\end{gathered}$$

These equations lead to the discriminating cubic for the determination of λ, and solution for l, m, n is then

$$\frac{l^2}{(\mathrm{Q}-\lambda)(\mathrm{R}-\lambda)-\mathrm{S}^2}=\frac{m^2}{(\mathrm{P}-\lambda)(\mathrm{R}-\lambda)-\mathrm{T}^2}=\frac{n^2}{(\mathrm{P}-\lambda)(\mathrm{Q}-\lambda)-\mathrm{U}^2}$$

In the case considered in the preceding sections S and U vanish, and the cubic reduces to the quadratic

$$(P-\lambda)(R-\lambda)-T^2=0$$

of which the solution is

$$2\lambda=P+R\pm\sqrt{(P-R)^2+4T^2}$$

m is obviously zero and l, n are determinable from

$$l^2(P-\lambda)=n^2(R-\lambda)$$

Let

$$l=\cos\vartheta,\quad n=\sin\vartheta$$

Then it is easily proved that

$$\cot 2\vartheta=\frac{P-R}{2T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

This equation gives the directions of the principal stress-axes.

The two principal stresses N_1, N_3 are the two values of λ, so that

$$\left.\begin{aligned}N_1&=\tfrac{1}{2}(P+R)+\tfrac{1}{2}\sqrt{(P-R)^2+4T^2}\\N_3&=\tfrac{1}{2}(P+R)-\tfrac{1}{2}\sqrt{(P-R)^2+4T^2}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

and the third principal stress, which we suppose intermediate in value between N_1 and N_3, is of course Q.

When an elastic solid is in a state of stress it is supposed, in all probability with justice, that the tendency of the solid to rupture at any point is to be estimated by the form of the stress quadric. At any rate the hypothesis is here adopted that the tendency to break is to be estimated by the difference between the greatest and least principal stresses. For the sake of brevity I shall refer to the difference between the greatest and least principal stresses as "the stress-difference." This quantity I shall find it convenient to indicate by Δ.

We may also look at the subject from another point of view:—It is a well-known theorem in the theory of elastic solids that the greatest shearing stress at any point is equal to a half of the stress-difference. It is difficult to conceive any mode in which an elastic solid can rupture except by shearing, and hence it appears that the greatest shearing stress is a proper measure of the tendency to break. This measure of tendency to break is exactly one-half of the stress-difference, and it is therefore a matter of indifference whether we take greatest shearing stress or stress-difference. For the sake of comparison with experimental results as to the stresses under which wires and rods of various materials will break and crush, I have found it more convenient to use stress-difference throughout; but the results may all be reduced to shearing stresses by merely halving the numbers given.

From (28) we have then

$$\Delta=\sqrt{(P-R)^2+4T^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

and the greatest shearing stress at the same point is $\frac{1}{2}\Delta$.

§ 4. *The application of previous analysis to the determination of the stresses produced by the weight of superficial inequalities.*

I have in a previous paper shown how Sir WILLIAM THOMSON'S solution for the state of internal strain of an elastic sphere subject to bodily forces, but not acted on by any surface forces, is to be adapted to the case of a spheroid (whose small inequalities are expressed as surface harmonics) of homogeneous elastic matter, endued with the power of mutual gravitation.[*] THOMSON'S solution is of course directly applicable for finding the state of strain due to a true external force, such as the tide-generating influence of the moon, but this forms only a part of the complete solution when the sphere has the power of gravitation. He introduced the effects of gravitation synthetically, but for my own purposes I prefer the analytical method pursued in my paper above referred to.

Suppose that $r=a+\sigma_i$ be the equation to an harmonic spheroid of the i^{th} order, forming inequalities on the surface of the sphere, whose density is w.

Then the causes producing a state of stress and strain in the mean sphere of radius a are, first a normal traction per unit area of the surface of the sphere equal to $-gw\sigma_i$, when g is the value of gravity, and secondly the attraction of the inequalities σ_i, acting throughout the whole sphere.

The first of these causes (viz.: the weight of the mountains or continents) is shown in my paper to produce the same state of strain as would be produced in the sphere, now free from surface action, by bodily forces corresponding with a potential $-gw(r/a)^i\sigma_i$.

As regards the second of these causes (viz.: the attraction of the mountains or continents), the potential of the layer of matter σ_i on any internal point, estimated per unit volume, is $3gw(r/a)^i\sigma_i/(2i+1)$.[†]

Then adding these two potentials together, we see that the surface inequalities σ_i produce the same state of strain as would be caused by the bodily forces due to a potential $-2(i-1)gw(r/a)^i\sigma_i/(2i+1)$, and the surface of the sphere is now subjected to no forces.

This expression is a solid harmonic of the i^{th} degree, and therefore the analytical results of the preceding sections are directly available for finding the state of stress due to continents and mountain chains.

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[*] "On the Bodily Tides of Viscous and Semi-elastic Spheroids, &c.," Phil. Trans., Part I., 1879, p. 1 (see § 2). This paper treats of the state of flow of a viscous sphere, but the problem is exactly the same as that concerning elasticity here considered. It is easy to see that if a viscous sphere be deformed into the shape of a zonal harmonic, the flow of the fluid must be meridional, and from this we may conclude that in the elastic sphere the plane of greatest and least principal stresses is also meridional. This has been already assumed to be the case in the present paper.

[†] If we could suppose a sphere to have homogeneous elasticity but heterogeneous density, this manner of building up the effective disturbing potential would have to be somewhat modified. Such an hypothesis is somewhat absurd, and I shall regard the sphere as homogeneous. In application to the case of the earth I shall however pay attention to the smaller density of the superficial layers by halving the height of the actual continents and the depth of the actual seas.

We must in fact put

$$W_i = -\frac{2(i-1)}{2i+1} gw \left(\frac{r}{a}\right)^i \sigma_i$$

Now in the previous developments the factors involving g, w, &c., have been omitted and W_i has been put equal to a zonal harmonic which had the value unity at the equator.

If we write

$$\mathfrak{s}_i = \sin^i\theta - \frac{i^2}{2\,!} \sin^{i-2}\theta \cos^2\theta + \&c. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

where θ is the colatitude, and put h as the height above the mean sphere of the elevation at the equator, then $h\mathfrak{s}_i = \sigma_i$ and

$$W_i = -\frac{2(i-1)}{2i+1} \frac{gwh}{a^i} (r^i \mathfrak{s}_i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

W_i was in (12) put equal to $r^i\mathfrak{s}_i$.

Thus in order to apply the preceding results to finding the stresses caused in a sphere, possessing the power of gravitation, by the weight and attraction of surface inequalities expressed by

$$r = a + h\mathfrak{s}_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

we must multiply the preceding results for P, R, T, Q by

$$-\frac{2(i-1)}{2i+1} \frac{gwh}{a^i} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

§ 5. *The state of stress due to ellipticity of figure or to tide-generating forces.*

When the effective disturbing potential W_i is a solid harmonic of the second degree the solution found above will give the stresses caused by oblateness or prolateness of the spheroid. It will of course also serve for the case of a rotating spheroid with more or less oblateness than is appropriate for the equilibrium figure.

When an elastic sphere is under the action of tide-generating forces the disturbing potential is a solid harmonic of the second degree, and therefore the present solution will apply to this case also.

The formula for the stress-difference admits of reduction to a simple form when $i=2$.

On substituting colatitude θ for latitude l, (22) gives

$$\begin{aligned} \text{P}-\text{R} &= r^2 \sin^2\theta[(A_0 - C_0) + (A_2 - C_2)\cot^2\theta] + a^2(B_0 - D_0) \\ \text{T} &= r^2 \sin\theta \cos\theta (E_0) \end{aligned}$$

Then substituting for A, B, C, D, E their values from Tables I., II., III., and making some simple reductions, we find

$$\left.\begin{aligned} \mathrm{P}-\mathrm{R} &= \tfrac{6}{19}[8(a^2-r^2)-r^2\cos 2\theta] \\ 2\mathrm{T} &= \tfrac{6}{19}r^2\sin 2\theta \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

Therefore by (29) in the present case, the stress-difference

$$\Delta = \tfrac{6}{19}\sqrt{64(a^2-r^2)^2+r^4-16r^2(a^2-r^2)\cos 2\theta} \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

In order to find the actual value of Δ in any special case we shall have to multiply (35) by appropriate factors; the factors will be determined below. For the present it will be more convenient to omit the factor $\frac{6}{19}$, and to reintroduce it along with these other factors.

The formula (35) enables us to determine the distribution of stress-difference throughout the sphere in the cases to which this section applies.

The curves of equal stress-difference are given by the equation

$$64(a^2-r^2)+r^4-16r^2(a^2-r^2)\cos 2\theta = \text{a constant}$$

The stress-difference at any point on any equatorial radius (for which $\theta=\frac{1}{2}\pi$) is clearly given by $8a^2-7r^2$, and along the polar radius (for which $\theta=0$) by $8a^2-9r^2$.

From this result it is clear that the stress-difference vanishes at that point on the polar radius for which $r=\frac{2}{3}a\sqrt{2}=\cdot 9425a$; this is the only point within the whole spheroid for which it vanishes.

When $r=a$ the stress-difference is equal to a^2, from which we obtain the remarkable result that the stress-difference is constant all over the surface. When $r=0$, it is equal to $8a^2$, which is eight times the surface value.

By means of arithmetical calculation and graphical interpolation I have drawn fig. 1; it shows the curves of equal stress-difference throughout a meridional section of the spheroid. The numbers written on the curves represent the values of stress-difference when the radius of the sphere is unity and when the factors above referred to are omitted (see Plate 19, fig. 1).

The point marked 0 is that in which the stress-difference vanishes. Round this point are drawn two dotted curves along which the stress-differences are $\frac{1}{2}$ and $\frac{3}{4}$ respectively. The remainder of the curves are drawn for equidistant values of stress-difference, and are marked 1, 2, 3 . . . 8. The curve marked 1 is singular, for the whole of the surface forms one branch of it, whilst there is another branch which runs below the surface from the polar axis and then rises to the surface at the point where $\cos 2\theta=-\frac{1}{8}$, that is to say, in lat. 41° 25′. Near the centre the curves are approximately circular, and they become somewhat like ellipses as we recede from the centre.

If this figure be made to rotate about the polar axis, the several curves will of course generate the surfaces of equal stress-difference throughout the sphere.

Writing ϑ for the inclination of one of the principal axes to the equator, we have by means of the formula (27)

$$\cot 2\vartheta=\frac{\mathrm{P}-\mathrm{R}}{2\mathrm{T}}=8\left\{\left(\frac{a}{r}\right)^2-1\right\}\operatorname{cosec} 2\theta-\cot 2\theta$$

It would be easy to trace out the changes of direction of the principal stress-axes throughout the sphere, but I will only now make the observation that all over the surface they are parallel to and perpendicular to the surface, and that at the centre they are polar and equatorial, the stress-quadric being of course an ellipsoid of revolution.

We have next to find the actual amount of stress-difference which arises from given ellipticity of form of the spheroid. Putting $i=2$ in (30) we have

$$\varsigma_i=\sin^2\theta-2\cos^2\theta=3[\tfrac{1}{3}-\cos^2\theta]$$

The equation to the spheroid is

$$\begin{aligned} r&=a+h\varsigma_i \\ &=a\left[1+3\frac{h}{a}(\tfrac{1}{3}-\cos^2\theta)\right]=a[1+e(\tfrac{1}{3}-\cos^2\theta)] \end{aligned}$$

Thus $3h/a$ is the ellipticity of the spheroid, which we may put equal to e.

Then it was shown in (33) § 4 that the results for the stresses P, Q, R, &c., are to be multiplied by $-\frac{2}{5}gwh/a^2$, and this will of course be also the factor for the stress-difference Δ.

Then substituting e for $3h/a$, and introducing the factor $\frac{6}{19}$, which has been omitted in considering the distribution of stress within the spheroid, we see that ellipticity e gives a stress-difference represented by

$$\Delta=-\frac{4e}{95}gwa\sqrt{64\left(1-\left(\frac{r}{a}\right)^2\right)^2+\left(\frac{r}{a}\right)^4-16\left(\frac{r}{a}\right)^2\left(1-\left(\frac{r}{a}\right)^2\right)\cos 2\theta}$$

If we estimate the forces in gravitation units the factor g must be omitted.

The expression under the square-root sign is equal to unity at the surface, and to 8 at the centre.

Thus the stress-differences, in gravitation units of force, at the surface and at the centre are $\frac{4}{95}ewa$ and $\frac{32}{95}ewa$ respectively.

To apply this to the case of the earth, take $a=637\times 10^6$ c.m., and $w=5{\cdot}66$, and we find the surface and central stress-differences to be respectively $152e$ and $1214e$ metric tonnes per square centimeter.

If these numbers be multiplied by 6·34, we get the same result expressed in tons per square inch. Thus in British units these two stress-differences are $926e$ and $7698e$.

If then the ellipticity e be $\frac{1}{1000}$th, the surface and central stress-differences will be nearly 1 ton and nearly 8 tons to the square inch respectively.

From the Table VII. in § 9 it will appear that cast brass ruptures with a stress-difference of about 8 tons to the square inch.

Thus a spheroid, made of material as strong as brass, and of the same dimensions and density as the earth, would only just support an excess or deficiency of ellipticity equal to $\frac{1}{1000}$th, above or below the equilibrium ellipticity adapted for its rotation.

The following is a second example:—If the homogeneous earth (with ellipticity $\frac{1}{232}$) were to stop rotating, the stress-difference at the centre would be 33 tons per square inch.

Now suppose the cause of internal stress to be the moon's tide-generating influence, and let $m=$ moon's mass, and $c=$ moon's distance.

Then the potential under which the earth is stressed is $-\frac{3}{2}(m/c^3)(\frac{1}{3}-\cos^2\theta)wr^2$, or according to the notation of § 4 $-\frac{1}{2}(m/c^3)wr^2\mathfrak{s}_2$.

If we took into account the elastic yielding of the earth and the weight and attraction of the tidal protuberance, this potential would have to be diminished. To estimate the diminution we must of course know the amount of elastic yielding, but as there is no means of approximating thereto, it will be left out of account.

Then it is obvious that the factor by which Δ, as given in (35), must be multiplied in order to give the stress-difference is $\frac{1}{2}mw/c^3$. Thus the surface stress-difference is $\frac{6}{19}\cdot\frac{1}{2}(m/c^3)wa^2$ in absolute force units.

Then putting M for the earth's mass, and dividing by gravity, we have $\frac{8}{19}(ma^3/Mc^3)wa$ as the surface value of Δ in gravitation units. The central value of Δ is of course eight times as great.

With the numerical data used above, $wa=3605$ metric tonnes per square c.m., and $m/M=\frac{1}{82}$, $a/c=\frac{1}{60}$, whence the surface stress-difference is 32 grammes, and the central stress-difference 257 grammes per square centimeter.

But this conclusion is erroneous for the following reason. If we suppose the moon to revolve in the terrestrial equator, and imagine that the meridian from which longitudes are measured is the meridian in which the moon stands at the instant under consideration, then the tide-generating potential is $-\frac{3}{2}(m/c^3)r^2[\frac{1}{3}-\sin^2\theta\cos^2\phi]$; this expression may be written $\frac{3}{4}(m/c^3)r^2(\frac{1}{3}-\cos^2\theta)+\frac{3}{4}(m/c^3)r^2\sin^2\theta\cos 2\phi$. The former of these terms produces a permanent increase of the earth's ellipticity, and is confused and lost in the ellipticity due to terrestrial rotation, and can produce no stress in the earth. The second term is the true tide-generating potential, but it is a sectorial harmonic, and I have failed to treat such cases. Now the first of these terms causes ellipticity in a homogeneous earth equal to $(\frac{5}{2}a/g)(\frac{3}{4}m/c^3)$ according to the equilibrium tide-theory. This ellipticity is equal to $\cdot1039\times10^{-6}$, an excessively small quantity. If however this permanent ellipticity does not exist (and the above investigation in reality presumes it not to exist), then there will be a superficial stress-difference equal to $152\times\cdot1039\times10^{-6}$ metric tonnes per square centimeter, and a central stress-difference of eight times as much.

Since a metric tonne is a million grammes this surface stress-difference is 16 grammes, and the central 128 grammes per square centimeter. These stress-differences are exactly the halves of those which have been computed above. Thus the remaining stress-difference which is due to the moon's tide-generating influence is 16 grammes at the surface and 128 grammes at the centre per square centimeter.

A flaw in this reasoning is that stress-difference is a non-linear function of the stresses, and therefore the stress-difference arising from the sum of two sets of bodily stresses is not the sum of their separate stress-differences.

I conceive however that the above conclusion is not likely to be much wrong.

These stresses are very small compared with those arising from the weights of mountains and continents as computed below, nevertheless they are so considerable that we can understand the enormous rigidity which Sir WILLIAM THOMSON has shown that the earth must possess in order to resist considerable tidal deformations of its mass.

§ 6. *On the stresses due to a series of parallel mountain chains.*

Having considered the case of the second harmonic, I now pass to the other extreme and suppose the order of harmonics i to be infinitely great, whilst the radius of the sphere is also infinitely great.

The equatorial belt now becomes infinitely wide, and the surface inequalities consist of a number of parallel simple harmonic mountains and valleys.

If i be infinitely large, we have from (12)

$$W_i=\rho^i\left[1-\frac{1}{2!}\left(\frac{iz}{\rho}\right)^2+\frac{1}{4!}\left(\frac{iz}{\rho}\right)^4-\&c.\right]$$

Now let ξ be the depth below the surface of the point indicated in the sphere (now infinitely large) by x, y, z.

As the formulas given above apply to the meridional plane for which $y=0$, we have $\rho=a-\xi$.

Now let $b=a/i$, then when both i and a are infinite

$$\rho^i=a^i\left(1-\frac{\xi}{a}\right)^i=a^i\epsilon^{-\xi/b}$$

and since in the limit $\rho/i=a/i=b$,

$$W_i=a^i\epsilon^{-\xi/b}\left(1-\frac{1}{2!}\frac{z^2}{b^2}+\frac{1}{4!}\frac{z^4}{b^4}-\&c.\right)$$

$$=(a^i)\epsilon^{-\xi/b}\cos\frac{z}{b}$$

This expression for W involves the infinite factor a^i, and in order to get rid of it we must now consider the factor by which it is to be multiplied, in introducing the height of the mountains and gravity.

This factor is computed in § 4; it is there shown that if $r=a+h\mathfrak{s}_i$ be a harmonic spheroid, the factor is $-2(i-1)gwh/(2i+1)a^i$.

Now if the harmonic i be of an infinitely high order, $\mathfrak{s}_i$ becomes simply $\cos z/b$, and the equation to the surface is

$$\xi=-h\cos\frac{z}{b}$$

ξ being measured downwards. Thus the harmonic spheroid $h\mathfrak{s}_i$ now represents a series of parallel harmonic mountains and valleys of height and depth h, and wave-length $2\pi b$.

The factor becomes $-gwh/a^i$, when i is infinite.

Thus the effective disturbing potential W, which is competent to produce the same state of stress and strain as the weight of the mountains and valleys, is given by

$$W=-gwh\epsilon^{-\xi/b}\cos\frac{z}{b} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

Now revert to the expressions (11) for the stresses.

When i is infinite $I=2i^2$, and they become, on changing x into $(a-\xi)$

$$P=\frac{1}{2i}(a^2-r^2)\frac{d^2W}{d\xi^2}+\frac{6(a-\xi)}{2i^2}\frac{dW}{d\xi}+\frac{3}{2i}W_i$$

$$T=-\frac{1}{2i}(a^2-r^2)\frac{d^2W}{d\xi dz}-\frac{3}{2i^2}\left((a-\xi)\frac{dW}{dz}-z\frac{dW}{d\xi}\right)$$

Now as shown above $a^2-r^2=2a\xi$, and $a/i=b$ in the limit; making these substitutions, and dropping the terms which become infinitely small when i is infinite, we have

and by a similar process

$$\left.\begin{array}{l} P=\xi b\dfrac{d^2W}{d\xi^2},\ T=-\xi b\dfrac{d^2W}{d\xi dz} \\ \\ R=\xi b\dfrac{d^2W}{dz^2},\ Q=0 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

Then from (36) and (37) we have

$$\left.\begin{array}{l} P=-gwh\dfrac{\xi}{b}\epsilon^{-\xi/b}\cos\dfrac{z}{b} \\ R=\quad gwh\dfrac{\xi}{b}\epsilon^{-\xi/b}\cos\dfrac{z}{b} \\ T=\quad gwh\dfrac{\xi}{b}\epsilon^{-\xi/b}\sin\dfrac{z}{b} \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

Since the stress-difference

$$\Delta=\sqrt{(P-R)^2+4T^2}$$

we have

$$\Delta=2gwh\frac{\xi}{b}\epsilon^{-\xi/b}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

The directions of the stress-axes are given by

$$\cot 2\vartheta = \frac{\mathrm{P}-\mathrm{R}}{2\mathrm{T}} = \cot \frac{z}{b}$$

so that

$$\vartheta = \tfrac{1}{2}\frac{z}{b} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

Equation (39) gives the stress-difference at a depth ξ below the mean surface, and is very remarkable as showing that the stress-difference depends on depth below the mean horizontal surface and not at all on the position of the point considered with reference to the ridges and furrows.

Equation (40) shows that if we travel along uniformly horizontally through the solid perpendicular to the ridges, the stress-axes revolve with a uniform angular velocity.

They are vertical and horizontal when we are under a ridge, and they have turned through a right angle and are again vertical and horizontal by the time we have arrived under a furrow.

Since the function $x\epsilon^{-x}$ is a maximum when $x=1$, the stress-difference Δ is a maximum when $\xi=b$,—that is to say, at a depth equal to $1/2\pi$ of the wave-length—and is then equal to $2gwh\epsilon^{-1}$ or in gravitation units of force to $\cdot 736\, wh$. It is interesting to notice that the value of this maximum depends only on the height and density of the mountains, and does not involve the distance from crest to crest. The depth at which this maximum is reached depends of course on the wave-length.

Plate 19, fig. 2, shows the distribution of stress-difference, the vertical ordinates represent stress-difference, and the horizontal ones depth below the surface. The numbers written on the horizontal axis are multiples of b; the distance OL on this scale is equal to 6·28, and is therefore equal to the wave-length from crest to crest, and the distance OH is the semi-wave-length from crest to furrow.

In the case of terrestrial mountains w is about 2·8, and if we suppose h to be 2000 meters, or a little over 6000 feet, we have the case of a series of lofty mountain chains —for it must be remembered that the valleys run down to 2000 meters below the mean surface, so that the mountains are some 13,000 feet above the valley-bottoms.

Then $h=2\times 10^5$, $w=2\cdot 8$, and the maximum stress-difference is

$$\cdot 736 \times 2\cdot 8 \times 2 \times 10^5 = \cdot 412 \times 10^6 \text{ grammes per square centimeter.}$$

This stress-difference is, in British measure, 2·6 tons per square inch.

If we suppose (as is not unreasonable) that it is 314 miles from crest to crest of the mountains, then the maximum stress will be reached at 50 miles below the surface.

From Table VII., § 9, it will be seen that if the materials of the earth at this depth of 50 miles had only as much tenacity as sheet lead, the mountain chains would sink down, but they would just be supported if the tenacity were equal to that of cast tin.

§ 7. *On the stresses due to the even zonal harmonic inequalities.*

Having considered the two extreme cases where i is 2, and infinity, I pass now to the intermediate ones. As the odd zonal harmonics were not required for the investigation in the following section I have only worked out in detail the even ones.

The surface of the sphere is now corrugated by a series of undulations parallel to the equator, and the altitude of the corrugations increases towards the poles. The form of the undulation in the neighbourhood of the equator is exhibited in Plate 19, fig. 3.

The stress-difference is as before given by

$$\Delta=\sqrt{(\mathrm{P}-\mathrm{R})^2+4\mathrm{T}^2}$$

To form this expression the series in (22) for R must be subtracted from the series for P. Since the C's and D's of Table II. have always the opposite signs from the A's and B's of Table I., this algebraic subtraction becomes a numerical addition of the numbers in these two tables.

The results are given in the following table.

TABLE IV.—The coefficients for expressing P—R.

i	A_0-C_0	A_2-C_2	A_4-C_4	A_6-C_6	B_0-D_0	B_2-D_2	B_4-D_4	B_6-D_6
2	−2·2105	−2·8421			+2·5263			
4	−4·3137	+2·7451	+7·8431		+4·3922	−7·5294		
6	−6·3636	+42	+35·6364	−14·2545	+6·4	−48·8727	+13·9636	
8	−8·3926	+139·1411	−38·8712	−168·2061	+8·4137	−148·0806	+188·4663	−21·5390
10	−10·4115	+318·2304	−633·7449	−478·0247	+10·4252	−329·2182	+965·7051	−491·6324
12	−12·425	+603·292	−2623·009	+552·212	+12·434	−616·315	+3243·765	−3806·018

Then we have

$$\begin{aligned}\mathrm{P}-\mathrm{R}=r^i\cos^i l[(A_0-C_0)+(A_2-C_2)\tan^2 l+\dots] \\ +a^2r^{i-2}\cos^{i-2} l[(B_0-D_0)+(B_2-D_2)\tan^2 l+\dots]\end{aligned}$$

The materials for computing T have been already given in Table III.

The series for P—R and for 2T should now be squared and added together, but the result would be so complex that it is not worth while to proceed algebraically.

At the equator T=0, and Δ=P—R, and P—R reduces to only two terms, whatever be the order of harmonic.

By reference to (23) and (24) we see that at the equator

$$\Delta=\frac{ir^{i-2}}{2(i+1)^2+1}\left[\frac{i(i+2)(2i-1)}{i-1}a^2-(i+1)(2i+3)r^2\right]$$

or

$$\Delta=\frac{i(i+1)(2i+3)a^2r^{i-2}}{(i+1)(2i+3)-i}\left[1-\left(\frac{r}{a}\right)^2+\frac{3}{(i^2-1)(2i+3)}\right] \quad . \quad . \quad . \quad . \quad . \quad (41)$$

This value for Δ requires of course multiplication by appropriate factors involving the height of the continents and gravity.

Even when i is no larger than 6, (41) differs but little from $ir^{i-2}(a^3-r^2)$, at least for values of r not very nearly equal to a.

Δ clearly reaches a maximum when

$$\left(\frac{r}{a}\right)^2=\frac{i-2}{i}\left\{1+\frac{3}{(i^2-1)(2i+3)}\right\}$$

For large values of i this maximum is nearly equal to $2\{(i-2)/i\}^{\frac{1}{2}i-1}a^i$

From these formulas the following results are easily obtained.

TABLE V. (a).

$i=$	2	4	6	8	10	12
Maximum value of Δ . . .	2·526	1·118	·959	·894	·859	·836
Value of r/a when Δ is max. .	0	·714	·819	·867	·895	·913

Plate 20, fig. 4, shows graphically the law of diminution of stress-difference for the even zonal harmonics, the vertical ordinates representing stress-difference and the horizontal ones the distances from the surface or from the centre of the globe.

In order to find a numerical value of these maximum stress-differences which shall be intelligible according to ordinary mechanical ideas, I will suppose the height of each of the harmonics at the equator to be 1500 meters. On account of the small density of the superficial layers in the earth, this is very nearly the same as supposing that in the earth the maximum height of the continents above, and the maximum depth of the depressions below the mean level of the earth are each about 3000 meters. In the summary at the end I shall show that there is reason to believe that this is about the magnitude of terrestrial inequalities.

Then by (33) we have to multiply the maximum stress-differences in the above table by $2(i-1)wh/(2i+1)$, in order to obtain the stress-differences for the supposed continents in grammes or tonnes per square centimeter.

Now $w=5{\cdot}66$ and $h=1{\cdot}5\times10^5$ according to the above hypothesis as to height of continent; and the coefficient in i is of course different for each harmonic.

By performing these multiplications I find the following results.

TABLE V. (*b*).—Maximum stress-differences due to harmonic continents and seas.

Order of harmonic.	2	4	6	8	10	12
Max. stress-difference, in metric tonnes per sq. c.m. due to continents 1500 meters high	·858	·633	·626	·625	·625	·625
Ditto in British tons per sq. inch, for same continents.	5·43	4·01	3·97	3·96	3·96	3·96
Depth in British miles at which this stress is attained	Centre of earth	1146	725	532	420	347

N.B.—The continents referred to are supposed to be of the earth's mean density and are equivalent to actual continents of double the height.

Thus far we have only considered the stress-differences at the equator immediately underneath the centres of the continents, but we must now see how they differ as the latitude of the place of observation increases. In order to attain this result a good deal of computation was necessary.

For this purpose I calculated P−R and 2T for a number of points and found the square root of the sum of these squares. As the computations were laborious, and as the results given in the following table are amply sufficient for the purpose in hand, I did not think it worth while to trace the changes to a greater depth than $r=\cdot 7$. Moreover the correctness of the last significant figures given cannot be guaranteed, although I believe that it is correct in most cases.

TABLE VI.—Showing the stress-difference due to the several harmonic inequalities at various depths and in various latitudes.

i		Equator.	Lat. 6°.	Lat. 12°.	i		Equator.	Lat. 6°.	Lat. 12°.
2	$r=1.$	·316	·316	·316	8	$r=1.$	·021	·015	·000
	$r=\cdot 9$	·736	·732	·721		$r=\cdot 9$	·859	·853	·853
	$r=\cdot 8$	1·112	1·108	1·097		$r=\cdot 8$	·798	·795	·797
	$r=\cdot 7$	1·443	1·440	1·431		$r=\cdot 7$	·506	·505	·507
4	$r=1.$	·079	·074	·061	10	$r=1.$	·014	·008	·007
	$r=\cdot 9$	·727	·719	·700		$r=\cdot 9$	·857	·854	·860
	$r=\cdot 8$	1·044	1·038	1·025		$r=\cdot 8$	·631	·630	·635
	$r=\cdot 7$	1·116	1·113	1·104		$r=\cdot 7$	·307	·307	·309
6	$r=1.$	·036	·031	·016	12	$r=1.$	·010	·003	·019
	$r=\cdot 9$	·817	·810	·800		$r=\cdot 9$	·827	·824	·835
	$r=\cdot 8$	·953	·949	·945		$r=\cdot 8$	·481	·481	·486
	$r=\cdot 7$	·788	·786	·785		$r=\cdot 7$	·179	·179	·181

The numbers given in the column marked "equator" might be computed from (41), and are those exhibited graphically in fig. 4; they are here given as a means of comparison with the numbers corresponding to latitudes 6° and 12°.

The result to be deduced from this table is that the lines of equal stress-difference are very nearly parallel with the surface, and that it is for all practical purposes sufficient to know the stress-difference immediately under the centre of the continents.

We have already seen in § 6 that for harmonics of infinitely high orders the lines of equal stress-difference are rigorously parallel with the mean surface.

§ 8. *On the stresses due to the weight of an equatorial continent.*

The actual continents and seas on the earth's surface have not got quite the regular wavy character of the elevations and depressions which have been treated hitherto. The subject of the present section possesses therefore a peculiar interest for the purpose of application to the earth. Had I however foreseen, at the beginning, the direction which the results of this whole investigation would take, it is probable that I might not have carried out the long computations which were required for discussing the case of an isolated continent. But now that that end has been reached, it seems worth while to place the results on record.

The function exp.$[-\cos^2\theta/\sin^2\alpha]$ (where θ is colatitude) obviously represents an equatorial belt of elevation, and I therefore chose it as the form of the required equatorial continent. This function has to be expanded in a series of zonal harmonics in order to apply the analytical solutions for the stresses produced by the weight of the continent.

It is obvious by inspection that the odd zonal harmonics can take no part in the representation of the function, and it was on this account that I have above only worked out the cases of the even zonal harmonics.

The multiplication of this function by the successive LEGENDRE'S functions, and integration over the surface of the sphere, are operations algebraically tedious, and wholly uninteresting, and I therefore simply give the results.

I find then that if $\alpha=10°$, and

$$\mathfrak{s}_i=\sin^i\theta-\frac{i^2}{2!}\sin^{i-2}\theta\cos^2\theta+\frac{i^2(i-2)^2}{4!}\sin^{i-4}\theta\cos^4\theta-\&c.$$

Then

$$\left.\begin{array}{l} 2\epsilon^{-\cos^2\theta/\sin^2\alpha}-\beta_0=\beta_2\mathfrak{s}_2+\beta_4\mathfrak{s}_4+\beta_6\mathfrak{s}_6+\beta_8\mathfrak{s}_8+\beta_{10}\mathfrak{s}_{10}+\beta_{12}\mathfrak{s}_{12}+\ldots \\ \text{where} \\ \beta_0=\cdot3078,\ \beta_2=\cdot3673,\ \beta_4=\cdot3339,\ \beta_6=\cdot2829,\ \beta_8=\cdot2252,\ \beta_{10}=\cdot1688, \\ \quad\beta_{12}=\cdot1193 \end{array}\right\} \quad . \quad (42)$$

β_0 is put on the left-hand side in order that the mean value of the function may be zero. The β's obviously decrease very slowly, and as I stop with the 12th harmonic, the representation of the function is very imperfect.

Plate 20, fig. 5, illustrates the results of the representation, the portion of a circle marked "mean level of earth" represents a meridional section of the earth; the dotted curve marked "inequality to be represented" shows the true value of the function $2\exp.[-\cos^2\theta/\sin^2\alpha]-\beta_0$; the curve marked "representation" shows the right-hand side of (42) stopping with the 12th harmonic; the second curve is the same without the 2nd harmonic constituent $\beta_2\varsigma_2$, and it is introduced for the reasons explained in the discussion and summary at the end.

The equatorial value of the exponential function is 1·792, that of the "representation" is 1·497, and that of "the representation without the 2nd harmonic" is 1·130.

The polar value of the exponential is $-$·3078, that of the "representation" is $-$·0830, and that of "the representation without 2nd harmonic" is $+$·6516. This latter function thus gives us an equatorial continent and a pair of polar continents of less height.

The extreme difference of height in the "representation" between the elevation at the equator and the depression at the pole is $(1·497+·083)h$ or 1·58h. I do not exactly know the extreme difference in the case where the 2nd harmonic is omitted, because I have not traced the inequality throughout, but it is probably about 1·3 or 1·4h.

Now let Δ_i be the numerical value, as computed for § 7, of the stress-difference due to the harmonic spheroid ς_i. Then it has been shown above that the stress-difference due to the spheroid whose equation is $r=a+h\varsigma_i$ is $-2(i-1)gwh\Delta_i/(2i+1)$.

Now stress-difference is a non-linear function of the component stresses P, R, T, and therefore the stress-difference due to a compound harmonic spheroid is not in general the sum of the stress-differences due to the constituent harmonic spheroids. At any point, however, where the principal stress-axes are all coincident in direction and where all the greater stress-axes coincide, and not a greater with a less, and where $T=0$, the stress-difference is linear and is the sum of the constituent stress-differences. This is the case at the equator for the present equatorial continent.

Hence, if Δ be the stress-difference at the equator due to the spheroid,

$$r=a+h(\beta_2\varsigma_2+\beta_4\varsigma_4+\ldots+\beta_{12}\varsigma_{12})$$

We have

$$\Delta=-gwh[\tfrac{2}{5}\beta_2\Delta_2+\tfrac{6}{9}\beta_4\Delta_4+\tfrac{10}{13}\beta_6\Delta_6+\tfrac{14}{17}\beta_8\Delta_8+\tfrac{18}{21}\beta_{10}\Delta_{10}+\tfrac{22}{25}\beta_{12}\Delta_{12}] \quad . \quad . \quad . \quad (43)$$

In this formula the Δ_i's are the numbers which were computed for drawing Plate 20, fig. 4, from the formula (41), namely

$$\Delta_i=-\frac{i(i+1)(2i+3)}{2(i+1)^2+1}\left(\frac{r}{a}\right)^{i-2}\left[1-\left(\frac{r}{a}\right)^2+\frac{3}{(i^2-1)(2i+3)}\right]$$

By using these computations I have drawn Plate 20, fig. 6. The vertical ordinates are $-\Delta\div gwh$, and the horizontal are the distances from surface or centre of the sphere.

The maxima in the two curves are merely found graphically, and the distances where the maxima are reached (viz.: 660 and 590 miles from the surface) are written down on the supposition that the radius of the sphere is 4000 miles.

In the discussion in the second part of this paper, I have endeavoured to make an estimate of the proper elevation to attribute to these isolated continents; so as to make the case, as nearly as may be, analogous to the earth.

Although it appears impossible to make an accurate estimate, I conclude that it will not be excessive if we assume that the greatest difference of height, between the highest point in the equatorial elevation and the approximately spherical remainder of the globe, is 2000 meters.

Accordingly for the representation I put $1\cdot58h=2000$, and for the second curve $1\cdot4h=2000$; these give $h=1\cdot27\times10^5$ c.m. and $h=1\cdot4\times10^5$ c.m. respectively.

Taking $w=5\cdot66$, then for the representation we have $wh=\cdot72$ tonnes per square centimeter, and for the other curve $wh=\cdot79$ of the same units. The maximum stress-differences are $\cdot91wh$ and $\cdot76wh$ respectively.

Therefore for the equatorial table-land (called above the representation) we have a maximum stress-difference of ·66 metric tonnes per square c.m. or 4·1 British tons per square inch; and for the equatorial table-land balanced by a pair of polar continents (2nd harmonic omitted) we have a maximum stress-difference of ·60 tonne per square c.m. or 3·8 tons per square inch.

I therefore conclude that our great continental plateaus produce a stress-difference of about 4 tons per square inch at a depth of 600 or 700 miles from the earth's surface.

§ 9. *On the strength of various substances.*

In order to have a proper comprehension of the strength which the earth's mass must possess in order to resist the tendency to rupture, produced by the unequal distribution of weights on the surface, it is necessary to consider the results of experiments.

RANKINE[*] gives a large number of results obtained by various experimenters, and Sir WILLIAM THOMSON also gives similar tables in his article on 'Elasticity.'[†]

Amongst other constants Sir WILLIAM gives YOUNG'S modulus and the greatest elastic extension. If the materials of a wire remain perfectly elastic when the wire is just on the point of breaking under tension, then the product of YOUNG'S modulus into the greatest elastic extension should be equal to what is called the tenacity, which is defined as the breaking tension per square centimeter of the area of the wire.

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[*] 'Useful Rules and Tables:' GRIFFIN, London, 1873, p. 191, *et seq.*

[†] 'Elasticity:' BLACK, Edinburgh, 1878. This is the article from the 'Encyclopedia Britannica,' but it is also published as a separate work.

If however a permanent set begins to take place before the wire breaks, this product should be less than the tenacity. I do not see how it can ever be greater, unless there be a marked departure from Hook's law "ut tensio sic vis;" or different sets of experiments with the same class of material might make it seem greater. In some of the results given by Sir William Thomson the product of modulus and elastic extension is however greater than tenacity.

Ordinary experience would lead one to suppose that such materials as lead and copper would undergo a considerable stress beyond the limits of perfect elasticity, before breaking. It is surprising therefore to see how nearly identical this product is to the tenacity—indeed in the case of lead absolutely identical, as may be seen in the table below.

With regard to the earth we require to know what is the limiting stress-difference under which a material takes permanent set or begins to flow, rather than the stress-difference under which it breaks; for if the materials of the earth were to begin to flow, the continents would sink down and the sea bottoms rise up.

It will be seen from the definition of tenacity given above that it is the rupturing stress-difference for tensional stresses. There is no word specially applied to rupturing stress-difference under pressure.

I am inclined to think that for the purposes of this investigation these tables in most cases rate the strength of materials somewhat too highly; for it seems probable that a permanent set would be taken, if a material were subjected for a long time to a stress-difference, which is a considerable fraction of the limiting value. We are likely to know more on this point in some years time when the wires hung by Sir William Thomson in the tower of Glasgow University have been subjected to several years of tension. However this may be I give the results of some of the experiments as collected and quoted by Sir William Thomson and the late Professor Rankine. The first table of tenacity, except the results denoted by the letter R, are taken from Sir William Thomson. The second table of crushing stress-difference is taken entirely from Rankine. The multiplications and reductions to different units I have done myself.

TABLE VII.—Limiting stress-difference.

Produced by tension.				Produced by crushing.		
Material.	Breaking stress-difference in metric tonnes per square centimeter.	Stress-difference at which permanent set begins in—		Material.	Breaking stress-difference in—	
		Metric tonnes per square centimeter.	British tons per square inch.		Metric tonnes per square centimeter.	British tons per square inch.
Sheet lead	·23	·23	1·46	Strong red brick .	·077	·49
Cast tin	·416	·417	2·64	Strong sandstone .	·39	2·45
,, ...	·325 (R)	..	2·06 (R)	(F) Strong limestone	·60	3·80
Wood (ash) . . .	1·20	1·20	7·61			
Cast brass	1·27	1·27	8·05	Marble	·39	2·45
,, iron	·94 to 2·04	1·14 to 1·87	7·23 to 11·86	Granite	·39 to ·77	2·45 to 4·91
Drawn copper . .	4·10	4·00	25·36	(F) Granite (Mount Sorrel)	·905	5·74
English steel pianoforte wire . . .	23·62	23·56	149·6	(F) Grauwacke . .	1·19	7·54
(R) Brick, cement .	·020 to ·021	..	·125 to ·134	Ash (along the grain)	·63	4·02
(R) Glass	·66	..	4·20	Cast brass . . .	·73	4·60
(R) Slate	·68 to ·90	..	4·3 to 5·7	Wrought iron . .	2·52 to 2·84	16 to 18

NOTE.—The second and third columns give the product of YOUNG's modulus into greatest elastic extension, and this should give the stress-difference when permanent set begins. RANKINE does not give the data for this quantity, but the breaking stress-difference is given in both metric and British units, the latter being in the third column.

In the second half of the table the results marked F are from Sir WILLIAM FAIRBAIRN's experiments.

The only cases in these two tables in which we have the opportunity of comparing the strength for resisting the stress-difference, when produced in the two manners, is for the materials cast brass and ash; in both cases we see that the substance is considerably weaker for crushing than for tension.

I should be inclined to suppose that the crushing strength is more nearly the datum we require for the case of the stresses in the earth.

In the first half of the table we probably see the effect of permanent set in the cases of copper and pianoforte wire (compare 4·00 with 4·10, and 23·56 with 23·62), but it is surprising that the contrast between the two columns is not more marked.

§ 10. *On the case when the elastic solid is compressible.*

It appears desirable to know how far the results of the preceding investigation may differ, if the elastic solid be compressible. According to the views of Dr. RITTER, referred to in the summary, this must be largely the case.

As I did not examine this point until after the above was completed, it seemed preferable to make a fresh beginning, rather than to modify the whole investigation.

The process is however so closely analogous, that it presents no difficulty, and may be dismissed shortly. I shall accordingly follow closely the processes of §§ 1 and 2.

If a solid be very compressible it takes a comparatively small hydrostatic pressure to produce a given amount of compression; that is to say, although the compressibility is large, the modulus of compressibility is small compared with that of rigidity. The modulus of compressibility I shall call the incompressibility. In the preceding investigation the converse was the case, for the incompressibility was infinite compared with the rigidity. By the definitions of ω and v in § 1, the incompressibility

$$k=\omega-\tfrac{1}{3}v.$$

It will be found convenient to use k and ω as the two moduli, which define the nature of the elastic solid.

In the denominators in E_i, F_i, G_i of (1), the expression $(2(i+1)^2+1)\omega-(2i+1)v$ occurs, this I shall call K, in analogy with I.

Then

$$K=2i(i-1)\omega+3(2i+1)k$$

If we develop the last differential coefficient in the expression (1) for α, we find

$$2vK\alpha=3k\left(\frac{i}{i-1}a^2-r^2\right)\frac{dW}{dx}+i\omega\left[(a^2-r^2)\frac{dW}{dx}+2xW\right] \quad . \quad . \quad . \quad . \quad (44)$$

Also

$$K\delta=iW \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

and

$$\left.\begin{aligned}\mathrm{P}&=(3k-2\omega)\delta+2v\frac{d\alpha}{dx}\\ \mathrm{T}&=v\left(\frac{d\alpha}{dz}+\frac{d\gamma}{dx}\right)\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

Differentiating (44) with regard to x, and substituting in (46) we find

$$K\mathrm{P}=3k\left[\left(\frac{i}{i-1}a^2-r^2\right)\frac{d^2W}{dx^2}-2x\frac{dW}{dx}+iW\right]+i\omega(a^2-r^2)\frac{d^2W}{dx^2} \quad . \quad . \quad . \quad (47)$$

Again, differentiating (44) with regard to z, writing down $d\gamma/dx$ by symmetry, and adding the two together, we have from (46)

$$K\mathrm{T}=3k\left[\left(\frac{i}{i-1}a^2-r^2\right)\frac{d^2W}{dxdz}-z\frac{dW}{dx}-x\frac{dW}{dz}\right]+i\omega(a^2-r^2)\frac{d^2W}{dxdz} \quad . \quad . \quad . \quad (48)$$

From (47) and (48) the other stresses may be written down by cyclic changes of letters.

Now let us suppose that the incompressibility is small compared with the rigidity.

Then ω becomes nearly equal to $\frac{1}{3}v$, and k is small compared with v and ω. Also K becomes nearly equal to $2i(i-1)\omega$. Hence in this case

$$\left.\begin{aligned} \mathrm{P} &= \frac{1}{2(i-1)}(a^2-r^2)\frac{d^2W}{dx^2} \\ \mathrm{T} &= \frac{1}{2(i-1)}(a^2-r^2)\frac{d^2W}{dxdz} \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (49)$$

Now if we suppose W to be a zonal harmonic of degree i, and only consider the state of stress at the equator, immediately underneath the centre of the elevation, then T is zero and by (15)

$$\mathrm{P} = \frac{i(i-1)}{2(i-1)}(a^2-r^2)r^{i-2}$$

$$\mathrm{R} = -\frac{i^2}{2(i-1)}(a^2-r^2)r^{i-2}$$

And

$$\Delta = \mathrm{P}-\mathrm{R} = \frac{i(2i-1)}{2(i-1)}(a^2-r^2)r^{i-2} \quad \ldots\ldots\ldots \quad (50)$$

If i be large $\Delta = i(a^2-r^2)r^{i-2}$.

In § 7 we have found this identical result, under the like conditions, when the solid is incompressible.

Now take the case of the 2nd harmonic, so that

$$W = r^2(\tfrac{1}{3}-\cos^2\theta) = \tfrac{1}{3}(x^2+y^2-2z^2)$$

And we have

$$\begin{aligned} \mathrm{P} &= \tfrac{1}{3}(a^2-r^2) \\ \mathrm{Q} &= \tfrac{1}{3}(a^2-r^2) \\ \mathrm{R} &= -\tfrac{2}{3}(a^2-r^2) \\ \mathrm{T} &= 0 \end{aligned}$$

Thus throughout the spheroid, the principal stress-axes are parallel and perpendicular to the polar axis; also

$$\Delta = (a^2-r^2)$$

Hence the central stress-difference is a^2. If the solid be incompressible it was found to be $8a^2$. Hence infinite compressibility largely relieves the stress-difference due to ellipticity of figure. Next take the case when the harmonic is of the 4th order.

Then at the equator we have by (50)

$$\Delta = \frac{4\cdot 7}{6}r^2(a^2-r^2) = \frac{14}{3}r^2(a^2-r^2)$$

The maximum is reached, when $r^2 = \frac{1}{2}a^2$ or $r = \cdot 707a$, and is equal to $\frac{7}{6} = 1\cdot 167$. Comparing this with the Table V. (a) § 7, we see that infinite compressibility makes very little difference, for ·707 differs little from ·714 and 1·167 from 1·118.

It will now be shown that whatever may be the compressibility of the solid, we get identically the same solution in the case when the harmonic is of an infinitely high order. This is the problem of the harmonic mountains and valleys corrugating a mean level surface, which was considered in § 6. The same notation will be adopted here.

Both i and a are infinite, x becomes $a-\xi$, and $ia^2/(i-1)-r^2=a^2-r^2=2a\xi$. If the substitutions here suggested be made in (47) and (48), it will be found that the terms with ω as a factor are multiplied by ai (two infinites), whilst none of the terms with k as a factor involve more than one infinite. Hence the latter terms are negligeable compared with the former.

Again i being infinite, $K=2i^2\omega$. Thus if i and a be infinite (47) and (48) reduce to

$$\mathrm{P}=\frac{1}{2i^2}(i2a\xi)\frac{d^2W}{d\xi^2},\ \mathrm{R}=\frac{1}{2i^2}(i2a\xi)\frac{d^2W}{dz^2},\ \mathrm{T}=-\frac{1}{2i^2}(i2a\xi)\frac{d^2W}{d\xi dz}$$

but $a/i=b$, therefore

$$\mathrm{P}=b\xi\frac{d^2W}{d\xi^2},\ \mathrm{R}=b\xi\frac{d^2W}{dz^2},\ \mathrm{T}=-b\xi\frac{d^2W}{d\xi dz}$$

In (36) we found the effective potential, which produced the same state of stress as the weight of the mountains whose equation was $-\xi=h\cos z/b$; the result was

$$W=-gwh\,\epsilon^{-\xi/b}\cos\frac{z}{b}$$

Hence in the present case

$$\mathrm{P}=-gwh\frac{\xi}{b}\epsilon^{-\xi/b}\cos\frac{z}{b},\ \mathrm{R}=gwh\frac{\xi}{b}\epsilon^{-\xi/b}\cos\frac{z}{b},\ \mathrm{T}=gwh\frac{\xi}{b}\epsilon^{-\xi/b}\sin\frac{z}{b}$$

These results are identical with equations (38), which gave the stresses when the elastic solid was incompressible.

It may be concluded from this that, except for the case of the 2nd harmonic inequality, compressibility makes very little difference, and for the higher harmonics it makes no difference at all.

II.

SUMMARY AND DISCUSSION.

The existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation.

Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials of which the earth is made must be strong enough to bear this stress.

We are thus led to inquire how the stresses are distributed in the earth's mass, and what are their magnitudes. These points cannot be discussed without an hypothesis as to the interior constitution of the earth.

In this paper I have solved a problem of the kind indicated for the case of a homogeneous incompressible elastic sphere, and have applied the results to the case of the earth.

It may of course be urged that the earth is not such as this treatment postulates.

The view which was formerly generally held was that the earth consists of a solid crust floating on a molten nucleus. It has also been lately maintained by Dr. AUGUST RITTER in a series of interesting papers that the interior of the earth is gaseous.* A third opinion, contended for by Sir WILLIAM THOMSON, and of which I am myself an adherent, is that the earth is throughout a solid of great rigidity; he explains the flow of lava from volcanoes either by the existence of liquid vesicles in the interior, or by the melting of solid matter, existing at high temperature and pressure, at points where diminution of pressure occurs.

There is another consideration, which is consistent with Sir WILLIAM THOMSON'S view, and which was pointed out to me by Professor STOKES. It may be that underneath each continent there is a region of deficient density; then underneath this region there would be no excess of pressure.

For the present investigation it is to some extent a matter of indifference as to which of these views is correct, for if it is only the crust of the earth which possesses rigidity, or if Professor STOKES'S suggestion of the regions of deficient density be correct, then the stresses in the crust or in the parts near the surface must be greater than those here computed—enormously greater if the crust be thin,† or if the region of deficient density be of no great thickness.

With regard to the property of incompressibility which is here attributed to the elastic sphere, it appears from §10 that even if we suppose the elastic solid to be *very* highly compressible, yet the results with regard to the internal stresses are almost the same as though it were incompressible. I think the hypothesis of great incompressibility is likely to be much nearer to the truth than is that of great compressibility. I shall therefore adhere to the supposition of infinite incompressibility, bearing in mind that even great compressibility would not much affect most of the results.

I take then a homogeneous incompressible elastic sphere, and suppose it to have the power of gravitation and to be superficially corrugated. In consequence of mathematical difficulties the problem is here only solved for the particular class of surface inequalities called zonal harmonics, the nature of which will be explained below.

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* 'Anwendung der mechanischen Wärmetheorie auf kosmologische Probleme.' CARL RÜMPLER, Hannover, 1879. This is a reprint of six papers in WIEDEMANN'S Annalen.

Dr. RITTER contends that the temperature in the interior of the planet is above the critical temperature and that of dissociation for all the constituents, so that they can only exist as gas. Data are wanting with regard to the mechanical properties of matter at, say 10,000° Fahr., and a pressure of many tons to the square inch. Is it not possible that such "gas" may have the density of mercury and the rigidity and tenacity of granite? Although such a conjectural "gaseous" solid might possess high rigidity, it almost certainly would have great compressibility: but it is proved in § 10 that the compressibility will make exceedingly little difference in the result of the present investigation excepting in the case of the 2nd harmonic inequality.

† The evaluation of the stresses in a crust, with fluid beneath, would be tedious, but not more difficult than the present investigation. I may perhaps undertake this at some future time.

Before discussing the state of stress produced by these inequalities, it will be convenient to explain the proper mode of estimating the strength of an elastic solid under stress.

At any point in the interior of a stressed elastic solid there are three lines mutually at right angles, which are called the principal stress-axes. Inside the solid at the point in question imagine a small plane (say a square centimeter or inch) drawn perpendicular to one of the stress-axes; such a small plane will be called an inter-face.* The matter on one side of the ideal inter-face might be removed without disturbing the equilibrium of the elastic solid, provided some proper force be applied to the inter-face; in other words, the matter on one side of an inter-face exerts a force on the matter on the other side. Now a stress-axis has the property that this force is parallel to the stress-axis to which the inter-face is perpendicular. Thus along a stress-axis the internal force is either purely a traction or purely a pressure. Treating pressures as negative tractions, we may say that at any point of a stressed elastic solid, there are three mutually perpendicular directions along which the stresses are purely tractional. The traction which must be applied to an inter-face of a square centimeter in area, in order to maintain equilibrium when the matter on one side of the inter-face is removed, is called a principal stress, and is of course to be measured by grammes weight per square centimeter.

If the three stresses be equal and negative, the matter at the point in question is simply squeezed by hydrostatic pressure, and it is not likely that in a homogeneous solid any simple hydrostatic pressure, *absolutely* equal in all directions, would ever rupture the solid. The effect of the equality of the three stresses when they are positive and tractional is obscure, but at least physicists do not in general suppose that this is the cause of rupture when a solid breaks.

If the three principal stresses be unequal, one must of course be greatest and one least, and there is reason to suppose that tendency of the solid to rupture is to be measured by the difference between these principal stresses.

In one very simple case we know that this is so, for if we imagine a square bar, of which the section is a square centimeter, to be submitted to simple longitudinal tension, then two of the principal stresses are zero (namely, the stresses perpendicular to the faces of the rod), and the third is equal to the longitudinal traction. The traction under which the rod breaks is a measure of its strength, and this is equal to the difference of principal stresses.

If at the same time the rod were subjected to great hydrostatic pressure, the breaking load would be very little, if at all affected; now the hydrostatic pressure subtracts the same quantity from all three principal stresses, but leaves the difference between the greatest and least principal stresses the same as before.

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* This term is due to Professor JAMES THOMSON.

Difference of principal stresses may also be produced by crushing.

In this paper I call the difference between the greatest and least principal stresses the "stress-difference," and I say that, if calculation shows that the weight of a certain inequality on the surface of the earth will produce such and such stress-difference at such and such a place, then the matter at that place must be at least as strong as matter which will break when an equal stress-difference is produced by traction or crushing.

I shall usually estimate stress-difference by metric tonnes (a million grammes) per square centimeter, or by British tons per square inch.

In Table VII., § 9, are given the experimentally determined values of the breaking stress-difference for various substances. The table is divided into two parts, in the former of which the stress-difference was produced by tension, and in the latter by crushing. It is not necessary here to advert to the difference in meaning of the numbers given in the first column and those given in the two latter columns in the first half of the table.

The cases of wood and cast brass are the only ones where a comparison is possible between the two breaking stress-differences, as differently produced. It will be seen that the material is weaker for crushing than for tension. For the reasons given in that section, I am inclined to think that these tables rate the strength of the materials somewhat too highly for the purposes of this investigation. I conceive that the results derived from crushing are more appropriate for the present purpose than those derived from tension; and fortunately the results for various kinds of rocks seem to have been principally derived from crushing stresses.

This table will serve as a means of comparison with the numerical results derived below, so that we shall see, for example, whether or not at 500 miles from the surface the materials of the earth are as strong as granite.

We may now pass to the mathematical investigation. It appears therefrom that the distribution of stress-difference is quite independent of the absolute heights and depths of the inequalities. Although the questions of distribution and magnitude of the stresses are thus independent, it will in general be convenient to discuss them more or less simultaneously.

The problem has only been solved for the class of superficial inequalities called zonal harmonics, and their nature will now be explained.

A zonal harmonic consists of a series of undulations corrugating the surface in parallels of latitude with reference to some equator on the globe; the number of the undulations is estimated by the order of the harmonic. The harmonic of the second order is the most fundamental kind, and consists of a single undulation forming an elevation round the equator, and a pair of depressions at the poles of that equator; it may also be defined as an elliptic spheroid of revolution, and the absolute magnitude is measured by the ellipticity of the spheroid.

If the order of the harmonic be high, say 30 or 40, we have a regular series of mountain chains and intervening valleys running round the sphere in parallels of latitude.

For the sake of convenience I shall always speak as though the equator were a region of elevation, but the only effect of changing elevations into depressions, and *vice versâ*, is to diametrically reverse the directions of all the stresses.

The harmonics of the orders 2, 6, 10, &c., have depressions at the poles of the sphere; those of orders 4, 8, 12, &c., have elevations at the pole.

The harmonic of the fourth order consists of an equatorial continent and a pair of circular polar continents, with an intervening depression. That of the sixth order consists of an equatorial continent and a pair of annular continents in latitudes (about) 60° on one and the other side of the equator. The 8th harmonic brings down these new annular continents to about latitude 45°, and adds a pair of polar continents; and so on.

By a continuation of this process the transition to the mountain chains and valleys is obvious.

In § 5 the case of the 2nd harmonic is considered. As above explained the sphere is deformed into a spheroid of revolution. The investigation also applies to the case of a rotating spheroid, such as the earth, with either more or less oblateness than is appropriate for the figure of equilibrium.

The lines throughout a meridional section of the spheroid along which the stress-difference is constant are shown in Plate 19, fig. 1, and the numbers written on the curves give the relative magnitude of the stress-difference.

It is remarkable that the stress-difference is the same all over the surface. In the polar regions the stress-difference diminishes as we descend into the spheroid and then increases again; in the equatorial regions it always increases as we descend. The maximum value is at the centre, and there the stress-difference is eight times as great as at the surface.

If the elastic solid be highly compressible the stress-differences are not nearly so great as on the hypothesis of incompressibility. In all the other cases considered in this paper compressibility makes practically no difference in the results.

On evaluating the stress-difference, on the hypothesis of incompressibility, arising from given ellipticity in a spheroid of the size and density of the earth, it appears that if the excess or defect of ellipticity above or below the equilibrium value (namely $\frac{1}{232}$ for the homogeneous earth) were $\frac{1}{1000}$, then the stress-difference at the centre would be 8 tons per square inch, and accordingly, if the sphere were made of material as strong as brass (see Table VII.), it would be just on the point of rupture. Again if the homogeneous earth, with ellipticity $\frac{1}{232}$, were to stop rotating, the central stress-difference would be 33 tons per square inch, and it would rupture if made of any material excepting the finest steel.

A rough calculation* will show that if the planet Mars has ellipticity $\frac{1}{80}$ (about twice the ellipticity on the hypothesis of homogeneity) the central stress-difference must be 6 tons per square inch. It was formerly supposed that the ellipticity of the planet was even greater than $\frac{1}{80}$, and even if the latest telescopic evidence had not been adverse to such a conclusion, we should feel bound to regard such supposed ellipticity with the greatest suspicion, in the face of the result just stated.

The state of internal stress of an elastic sphere under tide-generating forces is identical with that caused by ellipticity of figure.† Hence the investigation of § 5 gives the distribution of stress-difference caused in the earth by the moon's attraction. In Plate 19, fig. 1, the point called "the pole" is the point where the moon is in the zenith.

Computation shows that the stress-difference at the surface, due to the lunar tide-generating forces, is 16 grammes per square centimeter, and at the centre eight times as much. These stresses are considerable, although very small compared with those due to terrestrial inequalities, as will appear below.

In § 6 the stresses produced by harmonic inequalities of high orders are considered. This is in effect the case of a series of parallel mountains and valleys, corrugating a mean level surface with an infinite series of parallel ridges and furrows. In this case compressibility makes absolutely no difference in the result, as shown in § 10.

It is found that the stress-difference depends only on the depth below the mean surface, and is independent of the position of the point considered with regard to ridge and furrow; the direction of the stresses does however depend on this latter consideration.

In Plate 19, fig. 2, is shown the law by which the stress-difference increases and then diminishes as we go below the surface. The vertical ordinates of the curve indicate the relative magnitude of the stress-difference, and the horizontal ones the depth below the surface. The depth OL on the figure is equal to the distance between contiguous ridges, and the figure shows that the stress-difference is greatest at a depth equal to $\frac{1}{6 \cdot 28}$ of OL.

The greatest stress-difference depends merely on the height and density of the mountains, and the depth at which it is reached merely on the distance from ridge to ridge.

Numerical calculation shows that if we suppose a series of mountains, whose crests are 4000 meters or about 13,000 feet above the intermediate valley-bottoms, formed of rock of specific gravity 2·8, then the maximum stress-difference is 2·6 tons per square inch (about the tenacity of cast tin); also if the mountain chains are 314 miles apart the maximum stress-difference is reached at 50 miles below the mean surface.

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* The data for the calculation are: Ratio of terrestrial radius to Martian radius 1·878. Ratio of Martian mass to terrestrial mass ·1020. Whence ratio of Martian gravity to terrestrial gravity ·3596. Central stress-difference, due to ellipticity e, $996e$ tons per square inch. "Homogeneous" ellipticity of Mars $\frac{1}{166}$; and $\frac{996}{166}$ equal to 6.

† This is subject to certain qualifications noticed in § 5.

It may be necessary to warn the geologist that this investigation is approximate in a certain sense, for the results do not give the state of stress actually within the mountain prominences or near the surface in the valley-bottoms. The solution will however be very nearly accurate at some five or six miles below the valley-bottoms. The solution shows that the stress-difference is *nil* at the mean surface, but it is obvious that both the mountain masses and the valley-bottoms are in some state of stress.

The mathematician will easily see that this imperfection arises, because the problem really treated is that of an infinite elastic plane, subjected to simple harmonic tractions and pressures.

To find the state of stress actually within the mountain masses would probably be difficult.

The maximum stress-difference just found for the mountains and valleys obviously cannot be so great as that at the base of a vertical column of this rock, which has a section of a square inch and is 4000 meters high. The weight of such a column is 7·1 tons, and therefore the stress-difference at the base would be 7·1 tons per square inch. The maximum stress-difference computed above is 2·6, which is about three-eighths of 7·1 tons per square inch. Thus the support of the contiguous masses of rock, in the case just considered, serves as a relief to the rock to the extent of about five-eighths of the greatest possible stress-difference. This computation also gives a rough estimate of the stress-differences which must exist if the crust of the earth be thin. It is shown below that there is reason to suppose that the height from the crest to the bottom of the depression in such large undulations as those formed by Africa and America is about 6000 meters. The weight of a similar column 6000 meters high is nearly 11 tons.

In § 7 I take the cases of the even zonal harmonics from the 2nd to the 12th, but for all except the 2nd harmonic only the equatorial region of the sphere is considered.

Plate 19, fig. 3, shows an exaggerated outline of the equatorial portion of the inequalities; it only extends far enough to show half of the most southerly depression, even for the 12th harmonic. It did not seem worth while to trace the surfaces of equal stress-difference throughout the spheroid, but the laborious computations are carried far enough to show that these surfaces must be approximately parallel to the surface of the mean sphere. It is accordingly sufficient to find the law for the variation of stress-difference immediately underneath the equatorial belt of elevation. It requires comparatively little computation to obtain the results numerically, and the results of the computation are exhibited graphically in Plate 20, fig. 4.

Table V. (*b*), § 7, gives the maximum stress-differences, resulting from these several inequalities, computed under conditions adequately noted in the table itself. It will be convenient to postpone the discussion of the results.

In § 8 I build up out of these six harmonics an isolated equatorial continent. The nature of the elevation is exhibited in Plate 20, fig. 5, in the curve marked "representation;" no notice need be now taken of the dotted curve. This curve exhibits a belt of elevation of about 15° of latitude in semi-breadth, and the rest of the spheroid is approximately spherical. This kind of elevation requires the 2nd as one of its harmonic constituents, and this harmonic means ellipticity of the whole globe. Now it may perhaps be fairly contended that on the earth we have no such continent as would require a perceptible 2nd harmonic constituent. I therefore give in Plate 20, fig. 5, a second curve which represents an equatorial belt of elevation counterbalanced by a pair of polar continents in such a manner that there is no second harmonic constituent.

I have not attempted to trace the curves of equal stress-difference arising from these two kinds of elevation, but I believe that they will consist of a series of much elongated ovals, whose longer sides are approximately parallel with the surface of the globe, drawn about the maximum point in the interior of the sphere at the equator. The surfaces of equal stress-difference in the solid figure will thus be a number of flattened tubular surfaces one within the other.

At the equator however the law of variation of stress-difference is easy to evaluate, and Plate 20, fig. 6, shows the results graphically, the vertical ordinates representing stress-difference and the horizontal the depths below the surface. The upper curve in Plate 20, fig. 6, corresponds with the "representation curve" of Plate 20, fig. 5, and the lower curve with the case where there is no 2nd harmonic constituent.

The central stress-difference, which may be observed in the upper curve, results entirely from the presence of the 2nd harmonic constituent in the corresponding equatorial belt of elevation.

The maximum stress-differences in these two cases occur at about 660 and 590 miles from the surface respectively.

We now come to perhaps the most difficult question with regard to the whole subject—namely, how to apply these results most justly to the case of the earth.

The question to a great extent turns on the magnitude and extent of the superficial inequalities in the earth. As the investigation deals with the larger inequalities, it will be proper to suppose the more accentuated features of ridges, peaks, and holes to be smoothed out.

The stresses caused in the earth by deficiency of matter over the sea beds are the same as though the seas were replaced by a layer of rock, having everywhere a thickness of about $\frac{1 \cdot 02}{2 \cdot 75}$ or nearly $\frac{4}{11}$ of the actual depths of sea.

The surface being partially smoothed and dried in this manner, we require to find an ellipsoid of revolution which shall intersect the corrugations in such a manner that the total volume above it shall be equal to the total volume below it.

Such a spheroid may be assumed to be the figure of equilibrium appropriate to the earth's diurnal rotation; if it departs from the equilibrium form by even a little, then we shall much underestimate the stress in the earth's interior by supposing it to be a form of equilibrium.

Professor BRUNS has introduced the term "geoid" to express any one of the "level" surfaces in the neighbourhood of the earth's surface, and he endeavours to form an estimate of the departure of the continental masses and sea-bottoms from some mean geoid.* From the geodesic point of view the conception is valuable, but such an estimate is scarcely what we require in the present case. The mean geoid itself will necessarily partake of the contortions of the solid earth's surface, even apart from disturbances caused by local inequalities of density, and thus it cannot be a figure of equilibrium.

Thus, even if we were to suppose that the solid earth were everywhere coincident with a geoid—which is far from being the case—a state of stress would still be produced in the interior of the earth.

An example of this sort of consideration is afforded by the geodesic results arrived at by Colonel CLARKE, R.E.,† who finds that the ellipsoid which best satisfies geodesic measurement, has three unequal axes, and that one equatorial semi-axis is 1524 feet longer than the other. Now such an ellipsoid as this, although not exactly one of BRUNS' geoids, must be more nearly so than any spheroid of revolution; and yet this inequality (if really existent, and Colonel CLARKE's own words do not express any very great confidence) must produce stress in the earth. Colonel CLARKE's results show an ellipticity of the equator equal to $\frac{1}{13781}$, and this in the homogeneous elastic earth will be about equivalent to ellipticity $\frac{1}{27000}$; such ellipticity would produce a central stress-difference of $\frac{7698}{27000}$ or nearly one-third of a British ton per square inch.

From this discussion it may, I think, be fairly concluded that if we assume the sea-level as being the figure of equilibrium and estimate the departures therefrom, we shall be well within the mark.

The average height of the continents is about 350 meters (1150 feet), and the average depth of the great oceans is in round numbers 5000 meters (16,000 feet); but the latter datum is open to much uncertainty.‡ When the sea is solidified into rock the 5000 meters of depth is reduced to 3200 meters below the actual sea-level. Thus the average effective depression of sea-bed is about nine times as great as the average height of the land. I shall take it as exactly nine times as great, and put the depth as 3150 meters; but it is of course to be admitted that perhaps eight and perhaps ten might be more correct factors.

In the analytical investigation of this paper the outlines of the vertical section of the continents and depressions are always sweeping curves of the harmonic type, and the magnitude of the elevations and depressions are estimated by the greatest heights and depths, measured from a mean surface which equally divides the two.

We have already supposed the outlines of continents and sea-beds to have been smoothed down into sweeping curves, which we may take as being, roughly speaking, of the harmonic type. The smoothing will have left the averages unaffected.

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* 'Die Figur der Erde.' Von Dr. H. BRUNS. Berlin: STANKIEWICZ, 1878.

† Phil. Mag., Aug., 1878.

‡ In a previous paper, "Geological Changes, &c.," Phil. Trans., Vol. 167, Part I., p. 295, I have endeavoured to discuss this subject, and references to a few authorities will be found there.

The averages are not however estimated from a mean spheroidal surface, but from one which is far distant from the mean.

The questions now to be determined are as follows:—What is the proper greatest height and depression, estimated from a mean spheroid, which will bring out the above averages estimated from present sea-level, and what is the position of the mean spheroid with reference to the sea-level.

From the solution of the problem considered in the note below,[*] it appears that, if

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[*] Conceive a series of straight harmonic undulations corrugating a mean horizontal surface, and suppose them to be flooded with water. This will represent fairly well the undulations on the dried earth, and the water-level will represent the sea-level.

Suppose that the average heights and depths of the parts above and below water are known, and that it is required to find the position of the mean horizontal surface with reference to the water-level, and the height of the undulations measured from that mean surface.

Take an origin of coordinates in the water-level, the axis of x in the water-level and perpendicular to the undulations, and the axis of y measured upwards.

Let

$$y=h(\cos x-\cos a)$$

be the equation to the undulations.

The average height of the dry parts is clearly $\frac{1}{2a}\int_{-a}^{+a} y\,dx$ or $\frac{h}{a}(\sin a-a\cos a)$. Similarly the average depth below water is $\frac{h}{\pi-a}[\sin(\pi-a)-(\pi-a)\cos(\pi-a)]$ or $\frac{h}{\pi-a}\sin a+(\pi-a)\cos a$

If the latter average be p times as great as the former

$$ph\cos a\left(\frac{1}{a}\tan a-1\right)=h\cos a\left(\frac{1}{\pi-a}\tan a+1\right)$$

This is an equation for determining a.

Now I find that $a=34°\ 30'$ gives $p=8·983$, which corresponds very nearly with $p=9$ of the text above.

This value of a corresponds with an average equal to $·1165h$ for the height above water, and $1·0469h$ for the depth below water. Now if we put

$$1·0469h=3150 \text{ meters}$$
$$\text{which gives } ·1165h=350 \text{ meters very nearly,}$$

we have $h=3009$ meters.

The depth below water-level of the mean level is $h\cos 34°\ 30'$ or 2480 meters.

The greatest height of the dry part above the water-level is $3009-2480$ or 429 meters, and the greatest depth of the submerged part below water-level is $3009+2480$ or 5489 meters.

[After the proof-sheets of this paper had been corrected, Professor STOKES pointed out to me that, according to RIGAUD (Cam. Phil. Soc., vol. 6), the area of land is about four-fifteenths of the whole area of the earth's surface. Now, in the ideal undulations we are here considering the area above water is about one-tenth of the whole area; hence in this respect the analogy is not satisfactory between these undulations and the terrestrial continents. If I have not considerably over-estimated the average depth of the sea (and I do not think that I have done so), the discrepancy must arise from the fact that actual continents and sea-beds do not present in section curves which conform to the harmonic type; there must also be a difference between corrugated spherical and plane surfaces.

The geological denudation of the land must, to some extent, render our continents flat-topped.—*Added May 4th*, 1882.]

the continents and sea-beds have sections which are harmonic curves, then if we take,—

The mean level bisecting elevations and depressions as 2480 meters (8150 feet) below the sea-level, and the greatest elevation and depression from that mean level as 3009 meters (9840 feet), it results that the *average* height of the land above sea-level is 350 meters and the *average* depression of dried sea-bed is 3150 meters.

It thus appears that 3000 meters would be a proper greatest elevation and depression to assume for the harmonic analysis of this paper, if the earth were homogeneous. But as the density of superficial rocks is only a half of the mean density of the earth, I shall take 1500 meters as the greatest elevation and depression from the mean equilibrium spheroid of revolution.

It is proper here to note that the height of the undulations of elevation and depression in the zonal harmonic inequalities is considerably greater towards the poles than it is about the equator; it might therefore be maintained that by making 1500 meters the equatorial height, we are taking too high an estimate. But the state of stress caused in the sphere at any point depends very much more on the height of the inequality in the neighbourhood of a superficial point immediately over the point considered, than it does on the inequalities in remote parts of the sphere.

Now in all the inequalities, except the 2nd harmonic, I have considered the state of stress in the equatorial region, and it will therefore I think be proper to adhere to the 1500 meters for the greatest height and depression.

We have next to consider, what order of harmonic inequalities is most nearly analogous to the great terrestrial continents and oceans. The most obvious case to take is that of the two Americas and Africa with Europe. The average longitude of the Americas is between 60° and 80° W., and the average longitude of Africa is about 25° E., hence there is a difference of longitude of about a right-angle between the two masses. These two great continents would be more nearly represented by an harmonic of the sectorial class,* rather than by a zonal harmonic, nevertheless I think the solution for the zonal harmonic will be adequate for the present purpose.

Now it has been explained above that the harmonic of the fourth order represents an equatorial continent and a pair of polar continents. In the case of the 4th harmonic therefore there is a right angle of a great circle between contiguous continents. We may conclude from this that the large terrestrial inequalities are about equivalent to the harmonic of the fourth order.

Table V. (*b*), § 7, gives the maximum stress-differences under the centre of the equatorial elevation of the several zonal harmonics, the height of each being 1500 meters.

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* The sectorial harmonic of the fourth order $\sin^4\theta\cos 4\phi$ would well represent these two great continents. It would fairly represent China and Australia; but would annihilate the Himalayan plateau, and place another great continent in mid-Pacific. It is not at all difficult to find the stress-difference under the centre of a sectorial inequality, but to find it generally involves the solution of a cubic equation.

The point at which this maximum is reached is given in each case, and Plate 20, fig. 4, illustrates graphically the law of variation of stress-difference.

The second harmonic cannot be said to represent a continent, and the table shows that in each of the other cases the maximum stress-difference is very nearly 4 tons per square inch. The depths of the maximum point are of course very different in each case.

We have concluded above that Africa and America are about equivalent to an harmonic of the fourth order, hence it may be concluded that the stress-difference under those continents is at a maximum at more than 1100 miles from the earth's surface, and there amounts to about 4 tons per square inch. A comparison with Table VII. shows that marble would break under this stress, but that *strong* granite would stand.

The case of the isolated continent investigated in § 8 appeared likely to prove the most interesting one, for the purpose of application to the case of the earth. But unfortunately I have found it difficult to arrive at a satisfactory conclusion as to the proper height to attribute to the continent.

The average height of the American continent is about 1100 feet above the sea, and the average depth of the Pacific Ocean about 15,000 feet. If the water of the Pacific be congealed into rock, it will have an effective depth of 10,000 feet. The greatest height of the American continent above the bed of the dried Pacific when smoothed down must be fully 12,000 feet or 3700 meters. The height of the great central Asian plateau above the average bed of the southern ocean (after drying) must be considerably more than this.

Now in the application to the homogeneous planet the heights are to be halved to allow for the smaller density of surface rock.

I therefore take 2000 meters as the height of the top of the equatorial table-land above the remaining approximately spherical portion of the sphere.

The investigation of § 8 then shows that the equatorial table-land will give rise to a stress-difference of 4·1 tons per square inch at a depth of 660 miles; and that the equatorial table-land counterbalanced by the pair of polar continents (the second harmonic constituent being absent) gives a stress difference of about 3·8 tons per square inch at a depth of 590 miles.

This estimate of stress-difference agrees in amount, with singular exactness, with that just found from the case of the 4th zonal harmonic, but the maximum is reached 400 or 500 miles nearer to the earth's surface.

I think there can be no doubt but that there *are* terrestrial inequalities of much greater breadth than that of my isolated continent; thus this investigation for the isolated continent will give a position for the maximum stress-difference too near the surface to correspond with the largest continents. On the other hand, I do not feel at all sure that I have not considerably underestimated the height of such a comparatively narrow plateau.

In the present paper it has been impossible to take any notice of the stresses produced by the most fundamental inequality on the earth's surface, because it depends essentially on heterogeneity of density.

It is well known that the earth may be divided into two hemispheres, one of which consists almost entirely of land, and the other of sea. If the south of England be taken as the pole of a hemisphere, it will be found that almost the whole of the land, excepting Australia, lies in that hemisphere, whilst the antipodal hemisphere consists almost entirely of sea. This proves that the centre of gravity of the earth's mass is more remote from England, than the centre of figure of the solid globe.

A deformation of this kind is expressed by a surface harmonic of the first order, for such an harmonic is equivalent to a small displacement of the sphere as a whole, without true deformation. Now if we consider the surface forces produced by such a deformation in a homogeneous sphere, we find of course that there is an unbalanced resultant force acting on the whole sphere in the direction diametrically opposed to that of the equivalent displacement of the whole sphere.

The fact that in the homogeneous sphere such an unbalanced force exists shows that in this case the problem is meaningless; it is in fact merely equivalent to a mischoice in the origin for the coordinates. But in the case of the earth such an inequality does exist, and the force referred to must of course be counterbalanced somehow. The balance can only be maintained by inequalities of density, which are necessarily unknown. The problem therefore apparently eludes mathematical treatment.

It is certain that so wide-spreading an inequality, even if not great in amount, must produce great stress within the globe. And just as the 2nd harmonic produces a more even distribution of stress than the 4th, so it is likely that the first would produce a more even distribution than the 2nd.

It is difficult to avoid the conclusion that the whole of the solid portion of the earth is in a sensible state of stress.

I would not however lay very much emphasis on this point, because we are in such complete ignorance as to the manner in which the equilibrium of the solid part of the earth is maintained.

From this discussion it appears that if the earth be solid throughout, then at a thousand miles from the surface the material must be as strong as granite. If it be fluid or gaseous inside, and the crust a thousand miles thick that crust must be stronger than granite, and if only two or three hundred miles in thickness much stronger than granite. This conclusion is obviously strongly confirmatory of Sir WILLIAM THOMSON'S view that the earth is solid throughout.

V. *Experimental Determination of the Velocity of White and of Coloured Light.*

By JAMES YOUNG, *LL.D., F.R.S., and Professor* G. FORBES.

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PART I.

ABSOLUTE VELOCITY OF WHITE LIGHT.

Introductory.

IT is remarkable that, although the importance of an accurate knowledge of the velocity of light has been very generally appreciated, no attempt has hitherto been made in this country to measure that velocity by experiment. Our own experiments date from many years back, but we have been prevented by various interruptions to our work from giving a result which could lay claim to the greatest accuracy. In 1878 we made at Pitlochry, in Perthshire, between 600 and 700 observations, but the toothed wheel which was made for us not having the number of teeth in it which we had ordered, we were not able to eliminate perfectly certain unknown quantities occurring in the formulæ, and we felt that it would be better to wait until we could give a result in which we had perfect confidence. At the same time we resolved so to alter our apparatus that we should not have to depend upon the mean of a very large number of experiments to give us a good result, but that each observation should give us an accurate measurement, free from all doubt. This has now been accomplished by the experiments conducted in 1880–81 between Kelly House, Wemyss Bay, and the hills behind Innellan, across the mouth of the River Clyde.

The chief importance of a determination of the velocity of light is that it gives us the means, considered by many to be the best means, of determining the solar parallax, by combining the result with the constant of aberration determined by astronomers. The investigation has also acquired a further interest from the speculations of the late Professor CLERK MAXWELL, according to which the propagation of light is an electro-magnetic phenomenon, and its velocity should be the same as that of the propagation of an electro-magnetic displacement.

Our researches have, however, led us to results of great importance in another direction. We find reasons for believing that the different colours of which white light is composed do not travel with the same velocity, but that the more refrangible rays travel more rapidly through a vacuum, and that this difference is by no means very small, so that we may expect its presence to be determined by independent tests. The influence of this conclusion upon our views about the constitution of the ether is considerable, and will doubtless lead to some further knowledge of its properties.

The general theory of the method employed in the present research resembles that of M. FIZEAU. In his experiments, and in those of M. CORNU, a beam of light is sent through between the teeth of a toothed wheel to a distance of some miles, whence it is reflected back again by means of certain optical arrangements, which bring it back to the same point. If the wheel be rotating at a suitable rate, then, by the time that the light has gone to the distant station and back, the position of the space between two teeth is now occupied by a tooth and the light cannot pass. But if the velocity of the toothed wheel be doubled that position will be occupied by the next space and the light is able to pass. If the velocity be increased threefold, fourfold, &c., we have alternately eclipses and full brightness.

If m be the number of teeth in the wheel, it must, in the time that light has gone to the distant station and back again, have completed $\frac{1}{2m}$ of a revolution in order to bring a tooth into the position previously occupied by a space at the commencement of that time. If the wheel be found to be making N revolutions a second at the time of the first eclipse, then $\frac{1}{2m\mathrm{N}}$ is the fraction of a second taken by light to perform the double journey; and if the distance between the toothed wheel and the distant reflector be D then the velocity of light is

$$\mathrm{V}=4m\mathrm{ND}.$$

In the method which we devised, instead of having only one distant reflector, we have two, nearly in the same line, but one of them being at a greater distance than the other and a little to one side of it. Let us call the most distant reflector A and the other one B. The light reflected from A is eclipsed with a slower revolution of the toothed wheel than that from B; because the number of revolutions required is N, and we have

$$N=\frac{V}{4mD}.$$

But D_A (the distance to A) is greater than D_B (the distance to B); hence N_A (the speed of revolution producing the first eclipse with A) is less than N_B (the speed of revolution producing the first eclipse of B).

After the light from A has been eclipsed it begins to increase in brightness, while that from B is still diminishing. In the method of the present research we determine the speed of revolution when the two lights appear to be of equal brightness.

When we proceed to the second, third, &c., eclipses the difference in speed required to produce an eclipse in A and in B increases, and it may happen that at a certain speed the light from A reaches a maximum at the time when that from B is at a minimum, or *vice versâ*.

The superiority of this method over that of M. FIZEAU seemed to be that instead of having to determine the instant at which a light disappears we have only to determine the instant at which two lights seem to be of equal brightness. Every one who has been engaged in photometry is aware that the former is an operation of great difficulty and doubt, whereas the latter is one of very great delicacy.

The particular way in which we deduce the velocity of light from an observation of the equality of these two lights will now be explained. It must be especially noticed that we never have occasion to use the lights when near an eclipse, at which time, as CORNU has shown, irregularities are introduced into the formulæ.

We found that a great simplification was introduced in the formulæ by observing the 12th and 13th equalities (the ratio D_B to D_A being that of 12 to 13); so that two observations, one at each of these equalities, sufficed to give us a value of the velocity of light. We were also able to utilise some pairs of observations at the 13th and 14th equalities.

This method enabled us to use the electric light, which was in many ways the most convenient to us; for we are not dependent upon the absolute brightness of the light (which of course is liable to variations) but only on the proportionate brightness of the two reflected stars; and to prevent any error arising from any variation in this proportion (owing to fog, &c.), we never used a pair of observations which were separated in time by more than a few minutes.

Mathematical theory of our method.

If k be the width of a tooth, and $1-k$ be the width of the space between two teeth, and if E be the brightness of the star of light seen by reflection from the distant collimator when the toothed wheel is not in position, then the brightness of that star when the toothed wheel is in position and revolving slowly, but fast enough to cause the persistence of visual impressions to prevent us from distinguishing the separate teeth, is $E(1-k)$.

When the speed is increased the intensity depends upon the time taken by light to go to the distant reflector and to return. As the speed of the toothed wheel is gradually increased the star of light diminishes and increases in regular phases.

If the teeth be wider than the spaces the maximum light is less than $\frac{1}{2}E$, and the light is eclipsed not only at certain critical speeds, but also during a change of speed of rotation, which may be considerable if k be much greater than $\frac{1}{2}$.

If the spaces be wider than the teeth the maximum light is greater than $\frac{1}{2}E$, and the light is at no speed completely eclipsed, the minimum light being considerable if k be much less than $\frac{1}{2}$.

If the teeth of the wheel be not perfectly uniform, and if some teeth are wider than others, this will alter the law according to which the intensity varies with the speed of rotation. But, if the inequalities be not very great indeed, it is only in the neighbourhood of the maxima and minima that its influence is shown. The general effect is to increase the intensity of light at the minimum and to diminish it at the maximum.

If there be no very great irregularities in the width of the teeth, then (1) the *diminution* of intensity of the light with a definite increase of speed in the odd phases is perfectly constant, provided that in the interval of time taken by light to perform the double journey, the advancing part of each tooth of the wheel passes beyond the advancing part of the r^{th} tooth in front of it, and does not reach the following part of the $\overline{r+1}|^{\text{th}}$ tooth nor its following part reach the advancing part of the r^{th} tooth in front, where r is any whole number from zero upwards.*

Similarly, if there be no very great irregularities in the width of the teeth, then (2) the *increase* of intensity of the light with a definite increase of speed in the even phases is perfectly constant, provided that in the interval of time taken by light to perform the double journey the advancing part of each tooth in the wheel passes beyond the following part of the r^{th} tooth in front of it, and does not reach the advancing part of that tooth. This is in the condition where the width of a tooth is less than that of a space. If the width of a tooth be greater than that of a space

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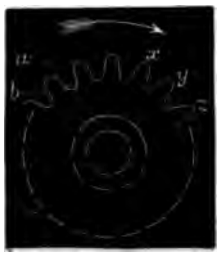
* We mean, by this notation, that if in the figure the arrow represents the direction of rotation of the toothed wheel a is the advancing and f the following part of that tooth, x, y, and z are the third, fourth, and fifth teeth in front of it.



the condition is that in the same interval of time the following part of each tooth should pass beyond the advancing part of the r^{th} tooth beyond it, but should not reach the following part of the $\overline{r+1}|^{\text{th}}$ tooth.

It is seen then that with a wheel which is carefully cut the rate of change of intensity of the light with change of speed of the toothed wheel is quite constant at that part of a phase which is about half way between the maximum and minimum brightness.

Our method of experimenting has reference to this part of a phase alone.

Let us call the period,—

From original brightness to the first extinction, the first phase;
From first extinction to next full brightness, the second phase;
From second full brightness to second extinction, the third phase;

and so on.

Let N be the number of revolutions per second made by the wheel when the first central eclipse is attained.

Let m be the number of teeth in the wheel.

Let D be the distance from the toothed wheel to the distant reflector.

Let I be the intensity of the star of light when the number of revolutions a second made by the toothed wheel is n.

Let V be the velocity of light.

Let τ be the time taken by light to perform the double journey, $\tau=\frac{2\mathrm{D}}{\mathrm{V}}$.

1. In the case of a wheel with teeth of the same width as the spaces (*i.e.*, $\kappa=\frac{1}{2}$), $\frac{d\mathrm{I}}{dn}$ is constant, then

$$\frac{d\mathrm{I}}{dn}=a;\ \mathrm{I}=an+\mathrm{C}$$

But if we be considering the first phase, then when $n=0$, $\mathrm{I}=\frac{1}{2}\mathrm{E}$; and when $n=\mathrm{N}$, $\mathrm{I}=0$.

Therefore

$$\mathrm{C}=\tfrac{1}{2}\mathrm{E},\ a=-\tfrac{1}{2}\frac{\mathrm{E}}{\mathrm{N}},$$

and

$$\mathrm{I}=-\tfrac{1}{2}\frac{\mathrm{E}}{\mathrm{N}}n+\tfrac{1}{2}\mathrm{E}$$

$$\frac{d\mathrm{I}}{dn}=-\tfrac{1}{2}\frac{\mathrm{E}}{\mathrm{N}}$$

2. In the case of a wheel with any width of teeth, the value of $\frac{d\mathrm{I}}{dn}$, in the middle of an odd phase, is the same as in the last example, but the maximum intensity is $\mathrm{E}(1-\kappa)$. Suppose that it is the p^{th} phase which we are considering, p being odd; then

$$\frac{dI}{dn}=-\tfrac{1}{2}\frac{E}{N}$$

$$I=-\tfrac{1}{2}\frac{E}{N}(n+b)$$

but when

$$n=(p-1)N,\ I=E(1-\kappa)$$

whence

$$b=a-N\{2(1-\kappa)+p-1\}$$

$$I=\frac{E}{2}\left\{2(1-\kappa)+p-1-\frac{n}{N}\right\}$$

3. In the case of a wheel with any width of teeth, the value of $\frac{dI}{dn}$, in the middle of an even phase, is the same as in the two previous examples in magnitude, but of opposite sign, and the maximum intensity is $E(1-\kappa)$. Suppose that it is the p^{th} phase which we are considering, p being even; then

$$\frac{dI}{dn}=+\tfrac{1}{2}\frac{E}{N}$$

$$I=\tfrac{1}{2}\frac{E}{N}(n+c)$$

but when

$$n=pN,\ I=E(1-\kappa)$$

whence

$$c=N\{2(1-\kappa)-p\}$$

and

$$I=\frac{E}{2}\left\{2(1-\kappa)-p+\frac{n}{N}\right\}$$

If we have two distant reflectors, A and B, nearly in the same line, but at different distances D_A and D_B, and having consequently different speeds N_A and N_B, and different intrinsic brightnesses E_A and E_B. Then, if D_A be greater than D_B, N_A is less than N_B, and we shall have equalities of brightness of A and B at different speeds. The first equality is in the first phase of B and the second of A. The second equality is in the second phase of B and the third of A, and so on. The r^{th} equality is in the r^{th} phase of B and the $\overline{r+1}|^{th}$ of A, provided that $r(N_B-N_A)$ is less than $2N_A$. We need not consider any other case.

4. Let us consider the case of the r^{th} equality when r is even. Here B is increasing, A is decreasing. We have then $p_A=r+1$, and $p_B=r$,

$$I_A=I_B;\ \text{or}\ \frac{E_A}{2}\left\{2(1-\kappa)+(r+1)-1-\frac{n}{N_A}\right\}=\frac{E_B}{2}\left\{2(1-\kappa')-r+\frac{n}{N_B}\right\}^{*}$$

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[*] We are taking account of the possibility of κ being different for A and B. But in practice this is never the case.

If $E_B = \rho E_A$ this becomes

$$2(1-\kappa)+r-\frac{n}{N_A}=\rho\left\{2(1-\kappa')-r+\frac{n}{N_B}\right\}$$

If ρ, κ and κ' were known we could determine the values of N_A and N_B from this equation when the velocity n giving equality of brightness had been determined, and when the distances D_A and D_B were known $\left(\text{for } \frac{N_A}{N_B}=\frac{D_B}{D_A}\right)$. But these quantities, ρ, κ, and κ', are not known.

5. Let us now consider the $\overline{r+1}|^{\text{th}}$ equality, when r has the same value as before. Here B is decreasing, A is increasing. We have $p_A = r+2$, $p_B = r+1$.

$$I'_A = I'_B\text{ ; or } \frac{E'_A}{2}\left\{2(1-\kappa)-(r+2)+\frac{n'}{N_A}\right\}=\frac{E'_B}{2}\left\{2(1-\kappa')+(r+1)-1-\frac{n'}{N_B}\right\}$$

Since E'_B still $=\rho E'_A$, this becomes

$$2(1-\kappa)-(r+2)+\frac{n'}{N_A}=\rho\left\{2(1-\kappa')+r-\frac{n'}{N_B}\right\}$$

6. From these two measurements, viz., of n and n', we can determine N_A and N_B in special circumstances. Let $\frac{D_A}{D_B}=g$. Then we have two equations

$$2(1-\kappa)+r-\frac{n}{N_A}=\rho\left\{2(1-\kappa')-r+\frac{n}{gN_A}\right\}$$

$$2(1-\kappa)-(r+2)+\frac{n'}{N_A}=\rho\left\{2(1-\kappa')+r-\frac{n'}{gN_A}\right\}$$

Subtracting, we have

$$2(r+1+\rho r)=\frac{1}{N_A}\left(\frac{\rho}{g}+1\right)(n+n')=\frac{g+\rho}{N_B}(n+n')$$

If now the distances D_A and D_B have been carefully chosen so as to make $g=\frac{r+1}{r}$, this equation becomes

$$2r(g+\rho)=\frac{g+\rho}{N_B}(n+n')$$

and

$$N_B=\frac{n+n'}{2r}$$

From this we can deduce the velocity of light. For

$$\tau_B=\frac{2D_B}{V}$$

and also

$$\tau_B=\frac{1}{2mN_B}$$

whence

$$V=4mN_B.D_B$$

$$=\frac{2m(n+n')D_B}{r}$$

7. If, however, $\frac{r+1}{r}$ be not quite exactly equal to g, let

$$\frac{r+1}{r}=g+\delta$$

our equation becomes

$$2r(g+\rho+\delta)=\frac{g+\rho}{N_B}(n+n')$$

and

$$N_B=\frac{n+n'}{2r}\cdot\frac{g+\rho}{g+\rho+\delta}$$

$$=\frac{n+n'}{2r}\left(1-\frac{\delta}{g+\rho}+\frac{\delta^2}{(g+\rho)^2}-\ldots\right)$$

and

$$V=\frac{2m(n+n')D_B}{r}\left(1-\frac{\delta}{g+\rho}+\frac{\delta^2}{(g+\rho)^2}-\ldots\right)$$

By employing the first term alone, in this last factor, we obtain an approximate value of N_B which enables us to calculate the value of ρ on the supposition that $\kappa=\kappa'=\frac{1}{2}$ (which is always very closely arranged so in practice). Thus we find the value of the small correction involved in the second term. The third term can always be neglected in our experiments.

8. To find the approximate value of ρ under these circumstances, we have now the two first equations in § 6, reduced to

$$1+r-\frac{gn}{N}=\rho\left(1-r+\frac{n}{N_B}\right)$$

and

$$1-(r+2)+\frac{gn'}{N_B}=\rho\left(1+r-\frac{n'}{N_B}\right)$$

Adding these we have

$$\frac{g}{N_B}(n'-n)=\rho\left\{2-\frac{1}{N_B}(n'-n)\right\}$$

and

$$\rho=\frac{g(n'-n)}{2N_B-(n'-n)}$$

also

$$g+\rho=\frac{2gN_B}{2N_B-(n'-n)}$$

The smallness of the correction due to the term $\frac{\delta}{g+\rho}$ in our experiments can be seen by examining the details of the observations.

In our experiments $g=\frac{13}{12}$ almost exactly. Most of our measurements were made with the 12th equality. Some however were made with the 13th equality. We will now examine the formula for this case.

9. Let us consider the case of the $\overline{r+2}|^{\text{th}}$ equality, r having the same value as before. Here B is increasing, A is decreasing. We have $p_A=r+3,\ p\ =r+2$.

$$\mathrm{I}''_A=\mathrm{I}''_B \text{ or } \frac{\mathrm{E}''_A}{2}\left\{2(1-\kappa)+(r+3)-1-\frac{n''}{\mathrm{N}_A}\right\}=\frac{\mathrm{E}''_B}{2}\left\{2(1-\kappa')-(r+2)+\frac{n''}{\mathrm{N}_B}\right\}$$

Since E''_B still $=\rho\mathrm{E}''_A$, this becomes

$$2(1-\kappa)+r+2-\frac{n''}{\mathrm{N}_A}=\rho\left\{2(1-\kappa')-(r+2)+\frac{n''}{g\mathrm{N}_A}\right\}$$

But at the $\overline{r-1}|^{\text{th}}$ equality

$$2(1-\kappa)-(r+2)+\frac{n'}{\mathrm{N}_A}=\rho\left\{2(1-\kappa')+r-\frac{n'}{g\mathrm{N}_A}\right\}$$

subtracting we have

$$2(r+2)+2\rho(r+1)=\frac{1}{\mathrm{N}_A}\left(1+\frac{\rho}{g}\right)(n'+n'')=\frac{g+\rho}{\mathrm{N}_B}(n'+n'')$$

If now D_A and D_B be so related that $g=\frac{r+2}{r+1}$, this equation becomes

$$2(r+1)(g+\rho)=\frac{g+\rho}{\mathrm{N}_B}(n'+n'')$$

$$\mathrm{N}_B=\frac{n'+n''}{2(r+1)} \text{ and } \mathrm{V}=\frac{2m(n'+n'')\mathrm{D}_B}{r+1}$$

If however $\frac{r+2}{r+1}=g+\delta'$ the true value of N_B is

$$\mathrm{N}_B=\frac{n'+n''}{2(r+1)}\left(1-\frac{\delta'}{g+\rho}+\frac{\delta'^2}{(g+\rho)^2}-\ \dots\right).$$

To approximate to the value of $g+\rho$ on the assumption that $\kappa=\kappa'=\frac{1}{2}$ we notice that the above equations reduce to

$$1+r+2-\frac{gn''}{\mathrm{N_B}}=\rho\left\{1-(r+2)+\frac{n''}{\mathrm{N_B}}\right\}$$

$$1-(r+2)+\frac{gn'}{\mathrm{N_B}}=\rho\left\{1+r-\frac{n'}{\mathrm{N_B}}\right\}$$

adding these we have

$$2-\frac{g}{\mathrm{N_B}}(n''-n')=\rho\left(\frac{n''-n'}{\mathrm{N_B}}\right)$$

and

$$\rho=\frac{2\mathrm{N_B}-g(n''-n')}{n''-n'}$$

also

$$g+\rho=\frac{2\mathrm{N_B}}{n''-n'}$$

Thus

$$\mathrm{V}=\frac{2m(n'+n'')\mathrm{D_B}}{r+1}\left\{1-\frac{\delta'(n''-n')}{2\mathrm{N_B}}+\ldots\right\}$$

when the 13th and 14th equalities are used.

Description of the apparatus.

The optical arrangements devised by M. **Fizeau** were admirably suited to the purpose, and were adopted, with modifications, by M. **Cornu.**

The telescope of emission, which is also the observing telescope, is pointed towards the distant stations. At its focus is placed the revolving toothed wheel; between that and the eye-piece is a diagonally-inclined piece of unsilvered glass.

The reflector designed by **Fizeau** for the distant station may be called a reflecting collimator; it consists of a telescope pointing towards the observing telescope, but, instead of having an eyepiece, it has in its principal focus a silver reflector. The advantages of this arrangement are its extreme simplicity and the facility of directing it—great accuracy of adjustment being unnecessary.

The rays of light coming from the sun, or any source of light,* are concentrated by means of a lens to throw an image of the source of light, after reflection at the diagonal mirror, upon the edge of the toothed wheel. If a tooth be not in the way these rays spread out and fill the object-glass, whence they proceed to the distant reflecting collimator, where they arrive all nearly parallel in direction. There they are caused by the object-glass to throw upon the focal mirror a luminous image of the object-glass of the observing telescope. The rays are then reflected to the object-glass, whence they proceed to the observing telescope and produce at its focus an image of the object-glass of the reflecting collimator. If no tooth be in the way, the rays proceed onwards through the eye-piece to the eye of the observer, who sees an illuminated image of the distant object-glass, which from its small size looks like a star of light. If now the toothed wheel revolves very slowly he sees the star eclipsed at intervals by the teeth; but if it be revolving so that at least ten teeth pass in a second, then, owing to the persistence of visual impressions he sees the star as a continuous light upon a brightish field produced by the illuminated rotating teeth. If the speed be further increased the brightness of the star diminishes owing to the light which passed between two teeth in leaving the observing station being partially stopped on its return by the advance of a tooth towards that space. So with increasing speed he sees the star disappear, then re-appear, attain its full brightness, diminish, disappear, reappear, &c., passing through similar phases with perfect regularity.

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* In the experiments of 1880–81 we always used the electric light.

One thing more is required either in the method of M. **Fizeau** or our own, and that is to have the means of determining at any instant the velocity of rotation of the toothed wheel. **Cornu** was the first to attempt to do this with accuracy: he connected the mechanism of the toothed wheel electrically with a chronograph so as to make a mark every 100 revolutions of the toothed wheel. A clock at the same time marked seconds, and by means of a vibrating spring tenths of a second were marked; while a fourth marker was under the control of the observer, who signalled the instant when he wished the velocity to be determined. **Cornu** did not attempt to maintain a uniform speed of revolution in the toothed wheel, but was able by means of the chronographic record to tell the velocity and rate of change of velocity at definite times, and hence, by interpolation, the exact velocity at any instant.

The plan of the present research was arranged in 1872, but it was not until 1875 that the apparatus was made. Since then the apparatus has been partially modified in order to overcome the optical and mechanical difficulties which arose in the course of the work. The most important pieces were—(1) the telescope, (2) the reflectors, (3) the revolving toothed wheel, (4) the clock, (5) the chronograph, (6) the dynamo-electric machine, and (7) the lamp.

1. *The telescope.*—This consists of a 5-inch achromatic object-glass of good quality, with a focal length of 7 feet. A **Bohnenberger's** eye-piece is employed, consisting of an erecting eyepiece with a piece of plain glass in front of the field lens and inclined to the axis of the telescope at an angle of 45°. A lateral hole in the tube of the eye-piece allows the light from a lamp, &c., at the side, to be reflected by this diagonal mirror along the axis of the telescope, and thence to the distant reflectors. The adapter, which connects the eye-piece with the body of the telescope, is a tube whose lower half is cut away so as to allow the revolving mechanism to be placed below in such a way as to bring the upper part of the revolving toothed wheel into the axis of the telescope and exactly in its focus. The light from a lamp or from the sun can be concentrated by a lens so as to throw its image upon the top of the toothed wheel. By looking along the axis of the telescope, from the object-glass, an observer can notice whether any of the light falls upon the inner sides of the telescope, and in this manner

he is able to adjust the direction of the beam of light so as to be central, in which case it must go directly to the distant reflectors whose images lie close together, not $\frac{1}{100}$th of an inch apart, on the teeth of the wheel.

Various optical difficulties presented themselves, and the arrangements were altered at various times. These chiefly consisted in illumination of the field of view, and were successively removed. Four improvements specially deserve notice:—

1. A general and intense illumination of the whole field was soon traced to a reflection from the centre of the object-glass. To obviate this a circle of black velvet about one inch in diameter was fastened to the centre of the object-glass on the inside. This was quite successful.

2. In using powerful lights an intense blaze was reflected from the toothed wheel, which made delicate observations impossible. We first tried smoking the toothed wheel, but not only did it still reflect a good deal of light but the regularity of form of the teeth was impaired; finally we used a highly-polished toothed wheel slightly bevelled, and by tilting the revolving mechanism the light was reflected to the upper parts of the interior of the adapter, which being blackened absorbed the light. This arrangement succeeded admirably, but some experiments were also made with the smoked wheel.

3. The field still had some general illumination owing to reflection from grains of dust, or scratches, or other imperfections in the plane-glass diagonal reflector. We objected to the glass reflector also for another reason: at an angle of 45° a *double layer* of glass reflects only 0·16 of the incident light; on the return of the light from the distant reflector 0·84 of this quantity passes through to the eye of the observer. The whole light obtained from this arrangement is then $0{\cdot}16 \times 0{\cdot}84 = 0{\cdot}136$; but if we could have an arrangement to reflect 0·5 of the light, so that on its return we should see $0{\cdot}5 \times 0{\cdot}5 = 0{\cdot}25$ of the light, we should nearly double the intensity. We accomplished this by substituting for the diagonal glass reflector a silvered reflector with an oval hole whose projection on a plane perpendicular to the axis of the telescope is a circle, and whose dimensions are such as to allow one-half of the light to pass through. The diagonal reflector is three inches from the toothed wheel. This arrangement certainly doubled the light, and owing to the darkness of the field its superiority over the glass reflector was enormous.

4. To prevent the slight illumination still remaining on the toothed wheel from causing inconvenience to the observer, a strip of metal with a series of holes of varying size was sometimes placed in the secondary focus of the eye-piece. It could be easily removed and replaced, so that when everything was prepared without its aid this diaphragm was inserted, using so small a hole as to show only the distant reflectors and two teeth of the wheel.

The toothed wheel.[*]—In contriving the revolving toothed wheel and the mechanism which gives it motion, considerable forethought is necessary. It must revolve at a very great speed, and must be capable of going at least for some minutes so as to avoid the necessity of continually winding it up. Our apparatus was constructed by Messrs. E. DENT and Co. It is mounted upon three screws, which rest in three holes countersunk in a plate of iron, which latter has an oblong hole in it to admit the passage of the catgut supporting the weight. This base plate of iron is, as before stated, generally slightly inclined by resting on a wedge, so that the light striking the toothed wheel is reflected upwards. Each wheel works into a pinion so as to multiply the velocity tenfold. These wheels and pinions are on five separate arbors, so that the multiplication is altogether 10,000-fold. On the first arbor the drum, 3 inches in diameter, is fixed. To the second a circular disc with a milled edge is attached. This enables the observer to increase or diminish the velocity of the wheel by a touch of the hand. It also enables him to reduce the velocity before putting on the brake, a sudden application of which might injure the mechanism. On the third arbor there is a cam which makes contact between two platinum points supported by two springs once in a revolution (*i.e.*, 100 revolutions of *the* toothed wheel), by means of which a current of electricity is transmitted to the chronographs. On the fourth arbor there is a brake consisting of a light disc of metal caught between two springs. To release this brake a handle or key behind the mechanism is turned through a right angle. This separates the springs and the brake ceases to act. The fifth arbor supports *the* toothed wheel, which is $1\frac{1}{2}$ inch in diameter, and contains 400 teeth cut to a depth of $\frac{1}{16}$th of an inch. The wheel is bevelled so as to reflect the light away from the observer's eye. Teeth of different shapes were tried, but the best was found to be that of saw teeth (*i.e.*, with pointed teeth and pointed spaces). The width of the teeth could then be varied by raising or lowering the mechanism by means of the foot screws. In the same way the toothed wheel could be brought exactly into the focus of the telescope. Wheels with varying numbers of teeth were also tried, and that one with 400 teeth was chosen as giving the best results.

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[*] The term "*the* toothed wheel" applies in this description to that toothed wheel in the mechanism between the teeth of which the light is made to pass.

The reflectors.—The two reflecting collimators are of identical construction. Those which we found to be most suitable were a pair constructed by Messrs. TROUGHTON and SIMMS. They are supplied with achromatic object-glasses of 3 inches diameter and 3 feet focus. At the other end of the tube a cap is screwed on, and to the centre of this cap a circular silver mirror is attached by three screws which admit of adjustment. This mirror is ground to a spherical form, the radius of the sphere being 3 feet, and the centre of the sphere of which it is a part being the centre of the object glass. Other collimators were tried, in which the object-glasses were replaced by 9-inch silvered glass reflectors of parabolic form. This pair was constructed by a maker who makes a speciality of such reflectors for telescopes; but the whole workmanship was so disgracefully bad in every part, every conceivable fault being found in it, and every known device for patching up and concealing bad work having been resorted to, that in the form supplied to us they were absolutely useless. They were discarded altogether when we found that the atmospheric conditions were seldom favourable enough to admit of our using a larger aperture than 3 inches. We have always used the 3-inch collimators in our observations. Three adjustments must be made: (1), for focus; (2), for centering; (3), for direction.

(1.) To adjust the focus, the cap with the mirror attached is removed and another one put in its place. This one is similar to it in every way except that the mirror is replaced by a piece of ground-glass whose surface has the same position with respect to the cap that the mirror has in the other one. The image of a star or the sun can be thrown upon the ground-glass and focussed. The number of turns and parts of a turn of the screw of the cap are then counted, and the cap with mirror is now screwed on to the same position. A final adjustment can be made with the collimators in position, altering the focus until an observer at the telescope sees the reflected light most distinctly.

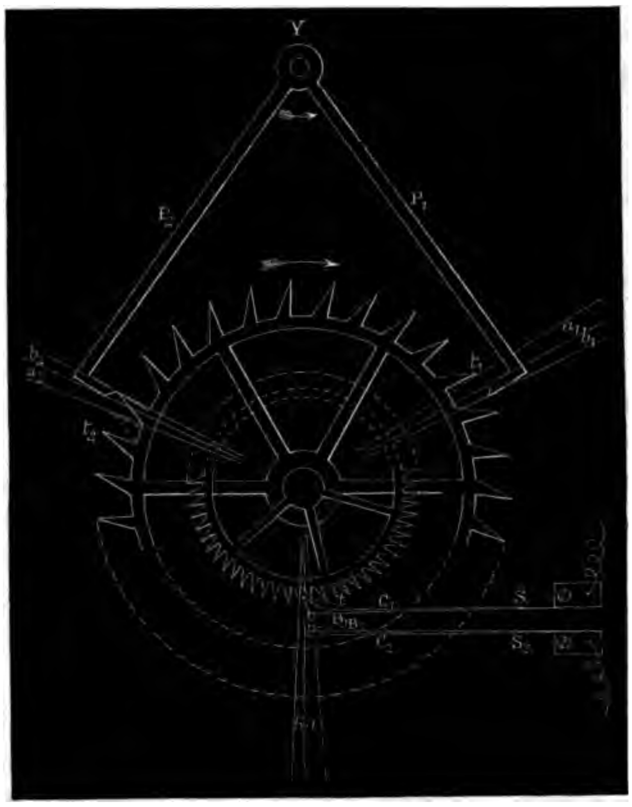
(2.) The adjustment which we call centering is accomplished by the three screws which attach the mirror to the cap, and causes the centre of the sphere of which the mirror forms a part to be at the centre of the object-glass. To test this point a tube 1 foot long is put on the collimator projecting in front of the object-glass. At the end of this tube a small ring is supported in the centre by three strips of metal. On looking through this ring the observer ought to see an image of his eye, and the screws are adjusted until this is the case.

(3.) To direct the collimators so that their axes should point to the observing telescope it was found most convenient to look through the object-glass at the mirror. If the head be so far as possible prevented from covering the object-glass an image of distant objects is seen in the mirror, and the direction is changed until the light coming from the observing telescope is seen in the centre of the mirror.

The clock.—The clock was constructed by Messrs. E. DENT and Co. specially for this research. It is driven by a weight attached to an endless chain passing over the drum. On the arbor of the drum, and connected with it by a toothed wheel and ratchet, is a wheel of 100 teeth working into a pinion of 10 teeth, which is on the same arbor as the scape wheel. This arbor bears a hand which marks seconds, and the arbour of the large wheel bears a hand which marks the minutes from 1 to 10. Any additional wheelwork was unnecessary for our purpose and would only tend to introduce errors. There is a dead-beat escapement, and the pendulum rod is made of varnished wood. The bob weighs 6 lbs., and the driving weight only 4 ozs., or one-twenty-fourth of the weight of the bob. The clock is best wound up by unhooking the weight from the endless chain after having hooked on another higher up. It goes without attention for two hours.

The arbor of the scape wheel bears another similar one, with 120 teeth, for making electrical contact once a second, by which means a mark shall be made on the chronograph. The system of electrical contacts adopted by us seemed to be free from the defects of those which tend to disturb the pendulum, because the springs whose contact sends the electric current are moved during the drop of the scape wheel from one pallet to the other. In the figure (fig. 1) a tooth has just passed from the impulse face of the pallet P_2. t_1 now falls through the angle a_1 on to the dead face of the pallet P_1. *During this interval* the tooth t of the smaller* wheel mounted on the same axis as the scape wheel moves the contact spring C_1 S_1 against the contact spring C_2 S_2, and the current passes. (The point of C_1 is agate, and the points of contact are platinum.) It will be seen that when the tooth t_1 goes on to give impulse to the pallet P_1 through the angle b_1 the tooth t has allowed the springs to separate and it passes through its angle b undisturbed. The motion of the tooth t_2 can be easily traced from the drawing. Careful workmanship was required to carry out this plan, as the angles a, a_1, a_2 ought to be made smaller than in the drawing. The result has been very successful.

Fig. 1.



Nevertheless, although this arrangement seems admirably adapted for not interfering with the regularity of the pendulum's vibration, we considered that it was not perfectly satisfactory for our purpose. For the length of a second marked by it on the chronograph would depend upon the exact form and size of the teeth in the auxiliary wheel. Consequently, we extemporised a contact free from this defect. A hole of the same size as the arbor of the crutch was bored in a piece of cork, and a slit was made in the cork to allow it to be fixed upon that arbor. Through the cork a thick copper wire was passed stretching horizontally for 3 inches, where it was bent downwards at a right angle. This bent arm was half an inch long, was amalgamated, and dipped, at each oscillation of the pendulum, into a cup of mercury connected with the electric circuit. The stout copper wire was also connected with that circuit by means of a very fine copper wire. This arrangement was very perfect.

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* The wheel is here drawn smaller for clearness of description. It is really of the same size.

The chronograph.—In all the experiments previous to 1880 we used one of the ingenious portable chronographs constructed by M. HYPP, of Neuchatel. In this instrument a strip of paper is run off from a drum, and upon it the signals from the clock and wheel are inscribed by two siphon pens. Uniformity of motion is given by the mechanism of the chronograph giving motion to a spur wheel against which the end of a spring presses. This spring permits one, two, &c., teeth of the wheel to pass in the course of a single vibration, the number allowed to pass being under control of the experimenter. A large number of observations were made with this apparatus. But we came to the decided opinion that while it is admirably adapted for observatory work where a greater accuracy than $\frac{1}{100}$th second is not required, yet in order to get the most perfect possible results a different class of instrument must be employed. Consequently we designed a new chronograph, which was constructed for us by Messrs. ELLIOTT Brothers. The principle of this apparatus is that we depend for uniformity of motion on the inertia of the apparatus. It was our object to get rid of all clockwork, and by making use of a fly-wheel, which has no work to do, to get rid of a host of irregularities which affect (it may be in a very slight degree) all other chronographs.

The base of the instrument consists of two strong triangular castings bound together in a horizontal position by three brass pillars. Between these triangles a fly-wheel, 12 inches in diameter, rotates on a vertical axis. The lower end of this axis is a hard steel socket which rests upon the point of a strong screw working in the centre of the lower triangle and firmly fixed there by means of a nut.

The lower triangle is supported upon three feet adjustable for levelling by screws. To the upper triangle two vertical brass pillars, 16 inches long, are fixed. They are connected at the top by a cross piece of iron, in the middle of which a thick pin works vertically with a screw motion, and it can be fixed by a nut.

The lower end of this pin terminates in a point, supporting the hard steel socket at the upper part of the axis of the registering cylinder. This cylinder is of brass. It is 12 inches long and 4 inches diameter. Its axis projects an inch at each end. The upper end, as before said, rests against the point of the upper screw. The lower end is, during an observation, firmly fixed to the axis of the fly-wheel by means of a solid brass tube closely fitting these two axes, with six screw nails to ensure rigidity. Round the upper end of the axis a silk thread is wound which, passing over a pulley mounted on friction wheels, carries a small weight. The axis of the cylinder is made vertical by the levelling screws, a spirit-level being placed on the top of the cylinder and observed in different positions of the fly-wheel. Thus we have a rigid mass rotating about a vertical axis with no cause for irregular motion except the acceleration produced by the weight and a slight friction, which combine to produce an accelerating or retarding force which may be regarded as constant. Two cylinders, exactly similar, were employed, and these could be interchanged. The brass cylinders were covered with a thin layer of smoke from a paraffin lamp.

The marks made by the currents of electricity from the clock and wheel respectively, were produced by two aluminium points attached to two springs, which were attracted by the poles of two electro-magnets in the circuits of the clock and wheel respectively. Thus, while the pens are in contact with the cylinder if there be no currents passing, and the fly-wheel be rotated, two circles are traced upon the cylinder. The electro-magnets are mounted on a stand which slides up and down one of the vertical brass pillars by means of a rack and pinion. Thus the assistant can, by turning the pinion, convert the two circular marks into spirals. We judged that it was much better to make an assistant perform this work than to use up the energy of the fly-wheel in the same way, as is often done in chronographs. Moreover, he turns the pinion only while an observation is being recorded, and not in every case when the fly-wheel is rotating, thus effecting a great saving in the space on the cylinder. The electro-magnets are not fixed rigidly to this support which moves up and down on the vertical column. They are attached to it by a vertical axis about which they have a small motion. Thus we are able to keep the pens in contact with the cylinder, or not, as we please. Ordinarily the pens are kept away from the cylinder by a light spring. By sending a current of electricity through a third electro-magnet, the pens are brought into contact with the cylinder. This current is sent by the observer by means of a contact maker, a few seconds before making an observation. When the assistant sees this he moves the pinion so as to cause the pens to describe a spiral, and the observer, at the instant of an observation, breaks and makes contact rapidly. This leaves a break on the smoked cylinder for about one-eighth of a second, the beginning of the break indicating the time of the observation.

When it is desired to read off the records of observations the cylinder is first taken off, and the collar which connected it rigidly to the fly-wheel removed. A ring, to which an arm carrying a vernier is attached, is now screwed to the centre of the upper triangle, and forms a bearing in which the axis of the fly-wheel can work. This part of the axis is conical, having the smallest diameter above. On this conical pivot a conical collar, which carries a 5-inch divided circle, is jammed. The cylinder is replaced with the lower end of its axis resting on the pointed upper end of the axis of the fly-wheel. It is prevented from rotating independently of the fly-wheel by an upright piece of metal and an upright spring, which are attached to the divided circle, and which catch between them one of four radial arms at the base of the cylinder.

It has been stated that one of the vertical pillars carries the marking pens or styles. The other pillar bears an arm to which is attached a microscope with cross wires in its focus. This microscope can be raised or lowered on the pillar which supports it. On looking through this microscope the marks made by one of the pens can be seen and brought, by rotating the fly-wheel, into super-position on the cross wires. The position of the cylinder is then read off by the vernier. The circle is divided into 500 equal parts, and by aid of the vernier we can read to one-twentieth part of one of these divisions, *i.e.*, to $\frac{1}{10000}$th of a revolution. We found it best in practice to rotate the apparatus at the rate of about one revolution a second. Hence one division of the vernier corresponds to about $\frac{1}{10000}$th of a second.

The perfect working of the chronograph depends upon a number of conditions which must all be fulfilled simultaneously. They are dependent (1) upon the manufacture of the instrument, (2) upon the mechanical adjustments, and (3) upon the electrical adjustments. The two last alone can be improved by the observer. The principal objects to be sought after in the manufacture of the instrument are absence of friction, rigidity, and the reduction of the moment of the momentum of the fly-wheel and cylinder together about their axis to zero.* The mechanical adjustments are as follows :—The vertical position of the axis of the instrument must be carefully tested by a level. The points of support must be oiled. The screws connecting the cylinder to the fly-wheel must be quite tight. The pressure of the upper point of support upon the axis must be very nicely adjusted. This point of support must be kept rigidly in position by the nut. The electrical adjustments are as follows :—The batteries and magnets must be in good working order. The pens must move freely and over a suitable range, and they must press with a suitable pressure upon the smoked cylinder.

The dynamo-electric machine.—The electric current to work our electric lamp was obtained from a small sized SIEMENS' dynamo-electric machine requiring three horse-power and rotating at the rate of 1,400 turns a minute. The axis of the machine is attached directly to a turbine of that kind known by the name of a vortex turbine. This was fed by a supply of water with a head of 300 feet coming from a distance of about a mile, and led to the house by a 3-inch water pipe. The current was led by means of stout wires from the generator to the lamp, a distance of about 100 feet. No further account of this machine is necessary, as our practice only, and not our results, could be affected by any imperfections.

The source of light.—We employed for our source of light, in the experiments of 1880–81, a SIEMENS' electric lamp. A condensing lens was used to throw an image of the incandescent carbon, after reflection by the diagonal reflector, upon the toothed wheel. Since with this lamp motion is given to only one of the carbon points it was necessary to mount the condensing lens upon a framework which should admit of its having an up and down movement. For the same reason the diagonal reflector in the eye-piece of the telescope was turned round about the axis of the telescope at each observation so as to send a maximum quantity of light along the axis to the distant reflectors. In some of our later experiments we used a bisulphide of carbon bottle-prism to send particular colours to the distant reflector. This prism was laid upon a ledge on the support of the condensing lens and immediately below it. With a little care a very pure spectrum could thus be formed.

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* The maker did not quite fully carry out our ideas on the two last of these points. The rigidity of the apparatus would have been increased by having three vertical pillars in place of two.

Establishment of the apparatus at Kelly, Wemyss Bay.

The room in Kelly House (the property of Dr. YOUNG) which was used for the observatory was the billiard room, facing the front of the house (the west). It is the room immediately to the north of the entrance hall or lobby. A pane of glass was removed from the window for observation. On the window-sill a brass plate has been fixed, with an inscription, indicating the exact relative position of the toothed wheel. The telescope was mounted upon two strong wooden supports. The toothed wheel mechanism rested upon two very solid beams (between which the cord for the weights could pass). These beams rested at one end upon a standard fixed to the wall of the room, and at the other end upon a strong box which rested on the billiard table. This end was also supported by a standard; and the floor was strengthened by thick wooden props below. The horizontal beams also acted as a support for the framework bearing the condensing lens and prism. The electric lamp was on a separate table. At one end of the beams and at the observer's right hand mercury cups were placed, with metallic contact pieces, by means of which the batteries could be put out of action when not required. LECLANCHÉ cells were used for the chronograph connexions. Twelve cells were used in all. The clock was attached to the wall of the room facing the observer. The levelling screws of the chronograph rested upon three metal plates on a piece of wood on the billiard table.

We have inserted a brass plate in the stone outside the window, with an arrow pointing towards the position occupied by the toothed wheel, the distance from the point being 106 inches.

Establishment of the reflecting collimators.—The reflecting collimators are placed on the hills behind the village of Innellan, separated from the observing telescope by the Firth of Clyde. The nearest one (B) is on the summit of the hill called *the Tom.** It rests upon two iron Y's which are imbedded in the solid rock. The reflector was 8 inches further from Kelly (*i.e.*, more to the west) than the most westerly of these two Y's, whose positions will always be marked by the holes. The collimator was covered by a wooden box with a hole in the east side. This box was attached to four iron rods placed in holes in the rock.

The more distant reflector (A) is in the face of the hill to the west of the Tom, nearly in the same line as a line drawn from Kelly to the top of the Tom, but two feet to the north of this line. It was impossible to gain a solid foundation, but the face of the hill was partially excavated, and two holes were "jumped" in a massive stone, in which two Y's were fixed to support the reflecting collimator. The reflector A was 8⅝ inches further from Kelly than the most westerly of these two holes.

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* Kelly House and *the Tom* are both shown on the Ordnance Survey Maps.

The reflector A was frequently shifted in direction (not in absolute position) by the unsatisfactory condition of the soil. This was especially the case during the severe frost of the winter, and on the occasion of the subsequent thaw. It was covered by a wooden box, open to the east.

The reflector A was not interfered with by any inquisitive persons. But the box covering B was once broken open at the west end, and the centreing screws abstracted, doubtless with the aim of obtaining a view of Kelly House with what was supposed to be a powerful telescope.

We made some sketches, and took some photographs of the apparatus, from which the accompanying plates have been prepared.

[*Note.*—It has been decided that the descriptions of the apparatus are sufficiently clear, and that it is unnecessary to supplement them by the plates to which allusion is here made.]

The general arrangement of our apparatus gave us considerable satisfaction. We could have wished, however, to have had a chronograph of the same kind, but more convenient in use in some ways, with greater rigidity, and with a smaller moment of momentum about the axis. The mechanism of the toothed wheel might perhaps be constructed so as to give a greater velocity than we were able to obtain. If a better climate were experienced considerable advantage would be gained, and the telescope and reflectors might then be increased in size, and the distances lengthened.*

†[*Measurement of the distances between the toothed wheel and the reflecting collimators.*

These measurements were made with the ordnance survey 20-inch theodolite. The base-line was the distance between the ordnance survey centre-marks on Knock Hill and Innellan Hill. The necessary information was furnished us by Colonel CLARKE. It seems unnecessary to give details of the triangulation, the manuscript being left in the charge of the Royal Society for reference. Calling Kelly House Station C, and the reflectors Stations A and B respectively, we obtained

$$\text{CA}=18{,}210\cdot 6 \text{ feet}$$
$$\text{CB}=16{,}825\cdot 3 \text{ feet.}]$$

These distances require several corrections.

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* We wish to record our indebtedness during the whole series of the observations which follow, to the able and skilful assistance rendered to us by Mr. D. STEWART, who superintended the working of the chronograph, and aided us in the work generally.

† Details of triangulation have been left out. The part in square brackets here is substituted.—G. F., December 17, 1881.

(1.) The distances of the reflectors and toothed wheel form the points used in the triangulation. These are

at A$-9{\cdot}18$ feet
at B$-0{\cdot}97$ foot
at C$+8{\cdot}83$ feet

giving a correction to

$$CA=-0{\cdot}35 \text{ foot}$$

and to

$$CB=+7{\cdot}86 \text{ feet.}$$

(2.) The thickness of the object-glasses, giving to CA and to CB a correction of $+0{\cdot}1$ feet to each.

These two corrections make CA$=18{,}210{\cdot}3$ feet and CB$=16{,}833{\cdot}3$ feet.

(3.) The reduction to a sphere whose radius is the distance of Kelly from the earth's centre instead of the sea level. The height of Kelly above the sea level is 100 feet. This correction is quite insensible.

(4.) The reduction of the circular measure of the angle subtended by CA or CB at the earth's centre to the chord of that arc. The angle is about 3′, and the reduction is quite insensible.

(5.) The station B is 414 feet higher than Kelly, and A is also in the same line. Owing to this cause there is a correction of $+1{\cdot}7$ feet to CB and $+1{\cdot}9$ feet to CA.

(6.) Lastly, the rays of light do not go in a straight line from C to A or B because they are bent into a curve by refraction. The curvature is so small that we may consider it as a part of a circle of very large radius. Supposing that the refraction amounted to half as much as the horizontal refraction of a heavenly body, *i.e.*, to 15′, then the angle subtended by this part of a circle would be 30′. But the difference between the circular measure of 30′ and the chord of the arc is insensible. Hence this correction is insensible.

The final result is that the distance CA$=18{,}212{\cdot}2$ feet and CB$=16{,}835{\cdot}0$ feet. Or $D_A=CA=3{\cdot}44{,}928$ miles. $D_B=CB=3{\cdot}18{,}845$ miles.

$$g=\frac{D_A}{D_B}=1{\cdot}08181$$

$$\delta=\frac{13}{12}-\frac{D_A}{D_B}=+0{\cdot}00152$$

$$\frac{r+1}{r}=\frac{13}{12}=1{\cdot}08333$$

$$\delta'=\frac{14}{13}-\frac{D_A}{D_B}=-0{\cdot}00484$$

$$\frac{r+2}{r+1}=\frac{14}{13}=1{\cdot}07697$$

Translation of the chronograph records.

The most usual way of observing with the chronograph was to complete the electric circuits for the clock and the rotating mechanism during the hour or two that we might be at work. The circuit which when closed brings the pens into contact with the smoked cylinder was however kept open, the means of closing it being a contact key in the hand of the observer. The assistant winds up the chronograph (which can only go for about 20 seconds) and levels it. He also examines the pens to see that they are in working order. The observer winds up the mechanism. He notices that his contact maker is in good working order. He oils the pivot holes of the mechanism. He puts on the number of weights which experience tells him will give the required speed approximately. He asks "Are you ready?" When the assistant is ready the observer releases the brake on the mechanism. He may if he please count the number of eclipses of A or B before his final speed is obtained. He adjusts the width of the teeth by raising or lowering the mechanism by means of one of the levelling screws. When the speed which gives equality of lights is nearly obtained he notices and records whether A or B is increasing with increase of speed. He makes the equality of lights more exact by adding to or taking from the driving weights, or by pulling the catgut supporting the weights either up or down, or by touching the milled head on the arbor of the second wheel on the mechanism, or in such a way as he has decided upon, until he feels that he has complete power to produce equality of lights. He then says to the assistant "Begin." The assistant gives a definite speed (judged by the eye) to the fly-wheel and cylinder of about one revolution a second. After a few seconds the observer assumes that the cylinder is rotating uniformly. He makes contact. The pens touch the smoked cylinder. The assistant immediately begins to lower the support of the pens by means of the rack and pinion, so as to make the pens describe spirals close to each other, the pens meanwhile marking seconds of the clock, and hundreds of revolutions of the toothed wheel respectively. The observer now adjusts the speed so as to produce equality of brightness in the two stars. When he is quite certain that he has attained this he breaks contact for one-eighth of a second or thereabouts. This leaves a blank in the spiral traces, the commencement of which indicates the time at which the speed is required.

The 12th and 13th equalities were generally used, and sometimes the 14th. Successive observations were always made at successive equalities to allow of the complete elimination of errors alluded to in the theory of our method.

The cylinder is completely covered after four or five observations have been made.

After the spirals have been traced by the clock-pen and the wheel-pen upon the smoked cylinder of the chronograph, the divided circle and vernier are attached to the instrument, the former being divided into 500 divisions, and the latter reading to $\frac{1}{10000}$th of a revolution. The microscope with its cross wires is placed so that the irregularities marked by the pens on the spiral traces can be readily seen, and brought to coincide in succession with the vertical wire. This done, a reading is taken, and the cylinder is turned round until another mark coincides with the vertical wire. So the operation is continued, first with the wheel marks, and afterwards with the clock marks. In reading off the marks on each side of a break in the traces care must be taken to notice whether a mark (or it may be even two marks) have been omitted. Care must also be taken to notice the number of whole revolutions of the cylinder. But it is only in the case of the clock-pen that this is necessary; and even in this case the omission of this precaution could hardly lead us into error. In this way we may obtain a series of readings for the wheel and clock respectively such as the following:—

January 20, 1881, No. 6.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
			515		
231			s_1 9,890	22,557	
	2,411	1,437	23,072	(22,419)	329
2,642			s_2 32,309	22,228	295
..	2,404	5,046	45,300	22,124	
7,449			54,433		
Signal at 3,810			Signal at 24,108		

The signals are indicated by the commencement of the break in the pen traces. Notice that in the reading of the wheel marks the third one is a blank, indicating that one mark is omitted in the break. In consequence of this the corresponding "difference" is half of the difference between the two numbers 2,642 and 7,449. Notice also that in taking the clock differences we must subtract the *alternate* readings, partly because the clock cannot be perfectly "in beat," and partly because the electrical connexion is such as to make the length of the second, as registered, depend upon the direction of vibration of the pendulum.

The "difference," such as 2,411, in the column of the wheel record, is the number of divisions (or ten-thousandths of a revolution) passed over by the cylinder in the course of 100 revolutions of the toothed wheel.

The "alternate difference," such as 22,419 in the column of the clock record, is the number of divisions passed over by the cylinder in the course of two seconds of time.

If the cylinder rotated with perfect uniformity the quotient of these two numbers, viz., $\frac{22419}{2411} \equiv 9{\cdot}2986$, would be the number of hundreds of revolutions completed by the toothed wheel in two seconds of time, and if we multiply this number by 50, we get 464·93 revolutions per second as the speed of the toothed wheel.

But as a matter of fact the chronograph-cylinder is constantly accelerated by the small weight, and retarded by the friction of the instrument. The total effect is generally a retardation, gradually diminishing the velocity of rotation of the chronograph. In the short interval of time (usually about one-fourth or one-fifth of a second) between two wheel marks this retardation does not affect us. We can say, in the above example, that the cylinder is passing over 0·2411 revolution in 100 turns of the toothed wheel, at the instant when the pen is at the reading 1,437; and that it is passing over 0·2404 revolution in 100 turns of the toothed wheel when the pen is at the reading 5,046. A simple proportion tells us that when the pen is at the reading of the signal, *i.e.*, 3,810, the number of revolutions in 100 turns of the toothed wheel is $0{\cdot}2411-\frac{2373}{3609}\times 0{\cdot}0007=0{\cdot}2406$.

But in the case of the clock trace we use intervals of two seconds. Here the case is very different. From the readings of the chronograph we must determine what was the actual velocity of the cylinder at some particular reading of the chronograph, what was the retardation produced by the excess of friction, what is the law according to which the velocity of rotation of the cylinder varies with the reading upon the cylinder; and thence what was the velocity of its rotation at the time when the signal was indicated.

Let s be the reading at any time t, and v the velocity of rotation of the cylinder.

When $t=0$, let $s=s_0$, and let $v=u$.

Let $s=s_1$ and s_2 when the times are t_1 t_2 respectively.

Let f be the excess of friction, or the retardation.

Then

$$s_1=s_0+ut_1-\tfrac{1}{2}ft_1^2$$

$$s_2=s_0+ut_2-\tfrac{1}{2}ft_2^2$$

$$s_2-s_1=u(t_2-t_1)-\tfrac{1}{2}f(t_2^2-t_1^2)$$

$$\bar{v}\equiv\frac{s_2-s_1}{t_2-t_1}=u-f\frac{(t_2+t_1)}{2}$$

Therefore $\bar{v}$=the velocity of rotation at the time $\frac{t_2+t_1}{2}$.

Now let $\bar{s}$ be the reading corresponding to the velocity of rotation $\bar{v}$, or the reading at that time $\frac{t_2+t_1}{2}$.

$$\bar{s}=s_0+u\frac{t_2+t_1}{2}-\tfrac{1}{2}f\left(\frac{t_2+t_1}{2}\right)^2$$

but

$$\frac{s_1+s_2}{2}=s_0+u\frac{t_2+t_1}{2}-\tfrac{1}{2}f\left(\frac{t_2^2+t_1^2}{2}\right)$$

Therefore

$$\bar{s}=\frac{s_1+s_2}{2}+\tfrac{1}{8}f(t_2^2+t_1^2-2t_1t_2)$$

$$=\frac{s_1+s_2}{2}+\tfrac{1}{2}f\left(\frac{t_2-t_1}{2}\right)^2$$

In our case $t_2-t_1=2$ seconds. Hence we have

$$\bar{s}=\frac{s_1+s_2}{2}+\tfrac{1}{2}f, \text{ corresponding to } \bar{v}=\frac{s_2-s_1}{2}.$$

To determine the value of f we notice above that

$$s_2-s_1=u(t_2-t_1)-\tfrac{1}{2}f(t_2^2-t_1^2)$$

so

$$s_3-s_2=u(t_3-t_2)-\tfrac{1}{2}f(t_3^2-t_2^2)$$

(where s_3 is the reading of the clock mark two seconds later than the mark indicated by s_2), but $t_2-t_1=t_3-t_2=2$ seconds. Therefore, subtracting, we have

$$\begin{aligned}(s_3-s_2)-(s_2-s_1)&=f(t_3-t_1)\\&=4f\end{aligned}$$

whence

$$f=\frac{(s_3-s_2)-(s_2-s_1)}{4}$$

It follows then that f is equal to the second alternate difference in the above example divided by 4.

This value of f, thus determined, is never perfectly constant; but we can interpolate so as to find its mean value in the interval between the reading $\frac{s_1+s_2}{2}+\frac{1}{2}f$ and the reading corresponding to the signal given by the observer.

[*N.B.*—When through imperfection of adjustment the values of f were very discordant, the observation was always rejected.]

To deduce the velocity v of rotation of the cylinder when the reading (corresponding to the signal) was s, we notice that

$$\begin{aligned}v^2&=\bar{v}^2-2f(s-\bar{s})\\&=\left(\frac{s_2-s_1}{2}\right)^2-2f\left(s-\frac{s_1+s_2}{2}-\tfrac{1}{2}f\right)\quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(A)}\end{aligned}$$

The first thing to be done in applying these formulæ is to determine the value of f which is to be adopted. The record given above shows us that, at the reading 23,072, $4f=329$; and at the reading 32,309, $4f=295$; and we obtain for the value of $4f$, at the signal reading 24,108, by simple proportion,

$$4f_1=329-\frac{1036}{9237}\times 34=325.$$

Taking this as a first approximation to the value of $4f$ which is to be adopted, we find

$$
\begin{aligned}
s_1 &= 9,890 \\
s_2 &= 32,309 \\
\hline
s_1+s_2 &= 42,199
\end{aligned}
$$

$\bar{s}=\frac{s_1+s_2}{2}+\frac{1}{2}f=21,100+41=21,141$ (assuming f_1 is sufficiently near to the correct value of f).

The value of $4f$ corresponding to this value of $\bar{s}$ is

$$4f_2=329+\frac{1931}{9237}\times 34\equiv 336.$$

The value for $4f$ which we must adopt is the mean of these two, $4f_1$ and $4f_2$.

$$4f=\tfrac{1}{2}(325+336)\equiv 331$$

[We might adopt this improved value for f to find a truer value for $\bar{s}$, and thence we might get a still nearer approximation to the correct value of f. In practice we find that no greater accuracy would be thus attained.]

Substituting now in equation (A) we have

$$\frac{s_2-s_1}{2}=11,210\,;\ s-\left\{\frac{s_1+s_2}{2}+\tfrac{1}{2}f\right\}=24,108-21,141$$

$$\equiv\ 2,967$$

$$v_2=(11,210)^2-166\times 2967$$

and

$$v=11,188.$$

We have neglected all account of the position of the decimal point up to this stage. It is easy to see that the above value of v means that the cylinder of the chronograph is, at the instant of equality of the lights, rotating at a speed which if uniform would accomplish 1·1188 revolutions per second. At the same instant the cylinder rotated 0·2406 of a revolution in the time taken by the toothed wheel to complete 100 revolutions. Hence the speed of rotation of the toothed wheel is

$$n'=\frac{1\cdot 1188}{0\cdot 002406}=465\cdot 00 \text{ revolutions per second.}$$

The value of v is calculated by the help of seven-figure logarithms.

Having now studied in detail the method of reduction of this particular observation, the following systematic form for the tabulation of results will easily be understood:—

January 20, 1881, No. 6.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
231 2,642 .. 7,449	2,411 2,404	1,437 5,046	515 9,890 23,072 32,309 45,300 54,433	22,557 (22,419) 22,228 22,124	329 295
Signal at 3,810			Signal at 24,108		
$v'=0{\cdot}002406$			$\bar{s}=21{,}141$ $4f=331$		
$w=\frac{v}{v'}=465{\cdot}00$			$\bar{v}=1{\cdot}1210$ Correction for friction, &c. $=-{\cdot}0028$ $v=1{\cdot}1182$		

Referring to the mathematical theory of our method it will be seen that in order to calculate for each observation the slight correction which must be applied, owing to $\frac{D_A}{D_B}$ being not exactly equal to $\frac{1}{2}\frac{3}{2}$, it is necessary to make a first approximation to the velocity of light neglecting this small correction. Our formula for this first approximation, studying the 12th and 13th equality (which is the best pair for our purpose), is

$$V=\frac{2m(n+n')D_B}{r}$$

The mean value of all our determinations of

$n+n'$ is $880{\cdot}02$	log $=2{\cdot}9444922$
$D_B=3{\cdot}18845$	„ ${\cdot}5035796$
$2m=800$	„ $2{\cdot}9030900$
	$6{\cdot}3511618$
$r=12$	„ $1{\cdot}0791812$
$\left\{\begin{array}{l}V=187{,}060\\ \text{first approximation}\end{array}\right.$	$5{\cdot}2719806$

Also from § 6 of the mathematical theory the first approximation to the value of $2N_B$ is $\frac{n+n'}{r}$

$\log(n+n')$	2·9444922
$\log r$	1·0791812
$2N_B=73{,}350$ . . . . . . first approximation	1·8653110
$g=1·08181$	·0341510
$2gN_B=79·3345$ . . . . . . first approximation	1·8994620

These are the values which are adopted in calculating the second term in the following reductions.

The rejection of observations.—We resolved never to reject a single observation simply because it differed largely from our average result. The only cases where we rejected an observation were (1) when there was no corresponding observation of the next equality taken at a very short interval of time, and (2) when the traces made by the clock pen on the chronograph showed great irregularities in the second differences, or when they showed that the friction on the axle was abnormally great.

With reference to No. (1) we may state that a quarter of an hour even might alter the value of ρ by fog, &c., and this would invalidate the result.

With reference to No. (2) it is right to say that we generally read off a large number of clock-pen marks corresponding to each observation. From these we could easily judge if the chronograph was working well.

December 21, 1880, No. 1.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
10,051			5,652		
	2,563	11,333	15,553	21,825	
12,614			27,477	21,714	254
	2,573	15,187	37,267	(21,571)	317
17,759			49,048	21,397	
			58,664		
Signal at 13,657			Signal at 33,915		
$v'=\cdot002569$			$\bar{s}=38{,}302$ $4f=\ 310$		
$n=\frac{v}{v'}=421\cdot08$			$\bar{v}=\ 1\cdot0786$ Correction for friction, &c. $=+\ \cdot0032$ $v=\ 1\cdot0818$		

December 21, 1880, No. 2.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
5,790			5,370		
	2,316	6,948	17,091	21,385	
8,106			26,755	(21,198)	294
	2,297	10,403	38,289	21,091	185
12,700			47,846	21,013	
			59,302		
Signal at 8,679			Signal at 28,958		
$v'=\cdot002306$			$\bar{s}=27{,}726$ $4f=\ 280$		
$n'=\frac{v}{v'}=459\cdot28$			$\bar{v}=\ 1\cdot0599$ Correction for friction, &c. $=-\ \cdot0008$ $v=\ 1\cdot0591$		

$$\text{From Nos. 1 and 2}\left\{\begin{array}{c} 2m\dfrac{(n+n')\mathrm{D_B}}{r}=187{,}132 \\ 1-\dfrac{\delta}{g+\rho}=0\cdot999323 \\ \text{Product }=\mathrm{V}=187{,}007 \end{array}\right\}\text{Correction for second term }=-125.$$

December 21, 1880, No. 5.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
			7,953		
1,197			17,419	16,742	
	1,964	2,179	24,695		294
3,161			..	(16,448)	
..	1,972	5,133	41,143		320
7,104			50,225	16,128	
			57,271		
Signal at 3,482			Signal at 33,692		
$v'=·001968$			$\bar{s}=32,958$ $4f=\quad 308$		
$n=\frac{v}{v'}=417·58$			$\bar{v}=\quad ·8224$ Correction for friction, &c. $=-\quad ·0006$ $v=\quad ·8218$		

December 21, 1880, No. 6.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
8,448			5,952		
	2,110	9,503	14,799	19,866	
10,558			25,818	19,699	339
..	2,083	12,641	34,498	(19,527)	282
14,723			45,345	19,417	
			53,915		
Signal at 12,241			Signal at 32,524		
$v'=·002086$			$\bar{s}=35,616$ $4f=\quad 285$		
$n'=\frac{v}{v'}=469·14$			$\bar{v}=\quad ·97640$ Correction for friction, &c. $=+\quad ·00224$ $v=\quad ·97864$		

$$\text{From Nos. 5 and 6}\left\{\begin{array}{c} 2m\frac{(n+n')D_B}{r}=188,484 \\ 1-\frac{\delta}{g+\rho}=0·999583 \\ \text{Product}=V=188,405 \end{array}\right\}\text{Correction for second term}=-79.$$

December 21, 1880, No. 8.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
7,785			7,927		
	1,843	8,707	17,479	17,425	
9,628			25,352	17,277	259
	1,825	11,453	34,756	(17,166)	304
13,277			42,518	16,973	
			51,729		
Signal at 10,263			Signal at 30,460		
$v'=\cdot 001833$			$\bar{s}=33{,}972$ $4f=\ \ 292$		
$n=\frac{v}{v'}=469\cdot 88$			$\bar{v}=\ \cdot 85830$ Correction for friction, &c. $=+\ \cdot 00298$ $v=\ \cdot 86128$		

December 21, 1880, No. 9.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences,
5,630			7,000		
	2,306	6,783	15,737	19,172	
7,936			26,172	(19,053)	244
	2,295	10,231	34,790	18,928	277
12,525			45,100	18,776	
			53,566		
Signal at 8,924			Signal at 29,096		
$v'=\cdot 002299$			$\bar{s}=25{,}295$ $4f=\ \ 248$		
$n=\frac{v}{v'}=413\cdot 32$			$\bar{v}=\ \cdot 95270$ Correction for friction, &c. $=-\ \cdot 00247$ $v=\ \cdot 95023$		

$$\text{From Nos. 8 and 9}\left\{\begin{array}{r} 2m\frac{(n+n')D_B}{r}=187{,}736 \\ 1-\frac{\delta}{g+\rho}=\cdot 999678 \\ \text{Product}=V=187{,}676 \end{array}\right\}\text{Correction for second term}=-60.$$

December 21, 1880, No. 10.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
3,542			9,604		
	2,253	4,669	19,223	21,151	
5,795			30,755	21,049	262
	2,258	8,053	40,272	(20,889)	287
10,310			51,644	20,762	
			61,034		
Signal at 6,918			Signal at 37,186		

$v'=\cdot002256$

$n'=\frac{v}{v'}=464\cdot24$

$\bar{s}=41{,}236$
$4f=\ \ 285$

$\bar{v}=\ \ 1\cdot0445$
Correction for friction, &c. $=+\ \ \cdot0028$
$v=\ \ 1\cdot0473$

From Nos. 9 and 10
$$\left.\begin{array}{r} 2m\frac{(n+n')\mathrm{D}_{\mathrm{B}}}{r}=186{,}537 \\ 1=\frac{\epsilon}{g+\rho}=\cdot999570 \\ \text{Product}=\mathrm{V}=186{,}457 \end{array}\right\}$$
Correction for second term $=-80$.

December 21, 1880, No. 11.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
1,897			2,422		
	2,824	3,309	13,039	23,235	
4,721			25,757	(23,190)	322
	2,826	7,547	36,229	23,013	255
10,373			48,770	22,935	
			59,164		
Signal at 5,690			Signal at 25,918		

$v'=\cdot002825$

$n=\frac{v}{v'}=410\cdot12$

$\bar{s}=24{,}675$
$4f=\ \ 325$

$\bar{v}=\ \ 1\cdot1595$
Correction for friction, &c. $=-\ \ \cdot0009$
$v=\ \ 1\cdot1586$

From Nos. 10 and 11
$$\left.\begin{array}{r} 2m\frac{(n+n')\mathrm{D}_{\mathrm{B}}}{r}=185{,}857 \\ 1-\frac{\epsilon}{g+\rho}=\cdot999632 \\ \text{Product}=\mathrm{V}=185{,}788 \end{array}\right\}$$
Correction for second term $=-79$

January 20, 1881, No. 3.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
9,610 12,666 18,767	3,056 3,051	11,138 15,717	6,415 21,264 31,805 46,456 56,885 71,344	25,390 (25,192) 25,080 24,888	310 304
Signal at 13,914			Signal at 34,168		
$v'=\cdot003053$			$\bar{s}=33,899$ $4f=309$		
$n=\frac{v}{v'}=412\cdot53$			$\bar{v}=1\cdot2596$ Correction for friction, &c. $=-\cdot0002$ $v=1\cdot2594$		

January 20, 1881, No. 4.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
2,214 4,960 10,469	2,746 2,755	3,587 7,715	7,554 22,621 33,322 48,207 58,791 73,563	25,768 (25,586) 25,469 25,356	299 230
Signal at 5,291			Signal at 35,490		
$v'=\cdot002750$			$\bar{s}=35,450$ $4f=289$		
$n'=\frac{v}{v'}=465\cdot18$			$\bar{v}=1\cdot2793$ Correction for friction, &c. $0\cdot0$ $v=1\cdot2793$		

From Nos. 3 and 4 $\left\{\begin{array}{r} 2m\frac{(n+n')D_B}{r}=186,569 \\ 1-\frac{\delta}{g+\rho}=\cdot999603 \\ \text{Product}=V=186,495 \end{array}\right\}$ Correction for second term $=-74$.

January 20, 1881, No. 5.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
2,926			8,863		
			22,476	23,264	
	2,761	4,307	32,127	(23,074)	369
5,687			45,550	22,895	408
	2,778	8,465	55,022	22,666	
11,243			68,216		
Signal at 6,119			Signal at 36,377		

$v' = \cdot 002768$

$n = \frac{v}{v'} = 415 \cdot 15$

$\bar{s} = 34{,}060$

$4f = 378$

$\bar{v} = 1 \cdot 1537$

Correction for friction, &c. $= - \cdot 0046$

$v = 1 \cdot 1491$

January 20, 1881, No. 6.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
231			515		
			9,890	22,557	
	2,411	1,437	23,072	(22,419)	329
2,642			32,309	22,228	295
	2,404	5,046	45,300	22,124	
7,449			54,433		
Signal at 3,810			Signal at 24,108		

$v' = \cdot 002406$

$n' = \frac{v}{v'} = 465 \cdot 00$

$\bar{s} = 21{,}141$

$4f = 331$

$\bar{v} = 1 \cdot 1210$

Correction for friction, &c. $= - \cdot 0022$

$v = 1 \cdot 1188$

From Nos. 5 and 6 $\left\{ \begin{array}{l} 2m\frac{(n+n')D_B}{r} = 187{,}088 \\ 1 - \frac{\delta}{g+\rho} = \cdot 999550 \\ \text{Product} = V = 187{,}003 \end{array} \right\}$ Correction for second term $= -85$.

December 21, 1880, No. 2

Wheel.			Clock.		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
5,790 8,106 . . 12,700	2,316 2,297	6,948 10,403	5,370 17,091 26,755 38,289 47,846 59,302	21,385 (21,198) 21,091 21,013	294 185
Signal at 8,679			Signal at 28,958		
$v'=\cdot002306$			$\bar{s}=27{,}726$ $4f=\ \ 280$		
$n'=\frac{v}{v'}=459{\cdot}28$			$\bar{v}=\ \ 1{\cdot}0599$ Correction for friction, &c. $=-\ \ \cdot0008$ $v=\ \ 1{\cdot}0591$		

December 21, 1880, No. 3.

Wheel.			Clock.		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
8,430 10,766 . . 15,400	2,336 2,317	9,598 13,083	9,432 21,958 32,432 . . 55,078 67,330 77,521	23,000 (22,646) 22,443	345 203
Signal at 12,051			Signal at 42,348		
$v'=\cdot002323$			$\bar{s}=43{,}790$ $4f=\ \ 282$		
$n''=\frac{v}{v'}=487{\cdot}89$			$\bar{v}=\ \ 1{\cdot}1323$ Correction for friction, &c. $=+\ \ \cdot0011$ $v=\ \ 1{\cdot}1334$		

$$\text{From Nos. 2 and 3}\left\{\begin{array}{r} 2m\,\frac{(n'+n'')\mathrm{D_B}}{r+1}=185{,}846 \\ 1-\frac{\delta'}{g+\rho}=1{\cdot}00185 \\ \text{Product}=\mathrm{V}=186{,}190 \end{array}\right\}\text{Correction for second term}=+344.$$

January 20, 1881, No. 6.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
231 2,642 .. 7,449	2,411 2,404	1,437 5,046	515 9,890 23,072 32,309 45,300 54,433	22,557 (22,419) 22,228 22,124	329 295
Signal at 2,406			Signal at 24,108		
$v'=\cdot 002406$			$\bar{s}=21{,}141$ $4f=\ 331$		
$n'=\frac{v}{v'}=465\cdot 00$			$\bar{v}=\ 1\cdot 1210$ Correction for friction, &c. $=-\ \cdot 0022$ $v=\ 1\cdot 1188$		

January 20, 1881, No. 7.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
6,229 8,610 .. 13,325	2,381 2,358	7,420 10,968	4,722 18,357 27,994 41,379 50,908 64,095	23,272 (23,022) 22,914 22,716	358 306
Signal at 9,144			Signal at 29,346		
$v'=\cdot 002370$			$\bar{s}=29{,}912$ $4f=\ 352$		
$n''=\frac{v}{v'}=485\cdot 87$			$\bar{v}=\ 1\cdot 1511$ Correction for friction, &c. $=+\ \cdot 0004$ $v=\ 1\cdot 1515$		

$$\text{From Nos. 6 and 7}\left\{\begin{aligned} 2m\frac{(n'+n'')D_B}{r+1}&=186{,}572\\ 1-\frac{\delta'}{g+\rho}&=1\cdot 00138\\ \text{Product }=V&=186{,}830\end{aligned}\right\}\text{Correction for second term }=+258.$$

January 20, 1881, No. 8.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
2,823 5,714 . . 11,528	2,891 2,907	4,269 8,621	139 16,186 27,469 43,326 54,535 70,190	27,330 (27,140) 27,066 26,864	264 276
Signal at 8,014			Signal at 28,248		
$v'=\cdot002905$			$\bar{s}=29{,}789$ $4f=\ \ 266$		
$n'=\frac{v}{v'}=467{\cdot}37$			$\bar{v}=\ \ 1{\cdot}3570$ Correction for friction, &c. $=+\ \ \cdot0007$ $v=\ \ 1{\cdot}3577$		

From Nos. 7 and 8 $\left\{\begin{array}{r} 2m\frac{(n'+n'')D_B}{r+1}=187{,}037 \\ 1-\frac{\delta'}{g+\rho}=1{\cdot}00122 \\ \text{Product}=V=187{,}266 \end{array}\right\}$ Correction for second term $=+229$

January 21, 1881, No. 2.					
Wheel.			**Clock.**		
Reading.	**Differences.**	**At mean reading.**	**Reading.**	**Alternate differences.**	**Alternate second differences.**
4,207 6,805 14,574	2,598 2,590	5,506 10,690	4,058 17,526 28,819 . . 53,165 65,962 77,117	24,761 (24,346) 23,952	415 394
Signal at 9,000			Signal at 39,234		
$v'=\cdot002593$			$\bar{s}=41{,}043$ $4f=\ \ 410$		
$n'=\frac{v}{v'}=470{\cdot}04$			$\bar{v}=\ \ 1{\cdot}2173$ Correction for friction, &c. $=+\ \ \cdot0015$ $v=\ \ 1{\cdot}2188$		

January 21, 1881, No. 3.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
7,503			5,879		
	2,446	8,726	16,947	23,799	
9,949			29,678	(23,672)	460
	2,412	12,361	40,619	23,339	462
. . 14,773			53,017	23,210	
			63,829		
Signal at 12,071			Signal at 32,305		
$v'=\cdot002415$			$\bar{s}=28{,}841$ $4f=\ \ 460$		
$n''=\frac{v}{v'}=487\cdot58$			$\bar{v}=\ \ 1\cdot1836$ Correction for friction, &c. $=-\ \ \cdot0061$ $v=\ \ 1\cdot1775$		

$$\text{From Nos. 2 and 3}\left\{\begin{array}{r} 2m\frac{(n'+n'')D_B}{r+1}=187{,}897\\ 1-\frac{\bar{c}'}{g+\rho}=1\cdot00116\\ \text{Product}=V=188{,}110\end{array}\right\}\text{Correction for second term}=+213$$

January 21, 1881, No. 4.

Wheel.			Clock.		
Reading.	Differences.	At mean reading.	Reading.	Alternate differences.	Alternate second differences.
7,376			4,725		
	2,517	8,635	15,768	23,609	
9,893			28,334	(23,449)	293
	2,483	12,376	39,217	23,316	245
. . 14,859			51,650	23,204	
			62,421		
Signal at 11,605			Signal at 31,786		
$v'=\cdot002940$			$\bar{s}=27{,}529$ $4f=\ \ 288$		
$n'=\frac{v}{v'}=469\cdot85$			$\bar{v}=\ \ 1\cdot1725$ Correction for friction, &c. $=-\ \ \cdot0026$ $v=\ \ 1\cdot1699$		

$$\text{From Nos. 3 and 4}\left\{\begin{array}{r} 2m\frac{(n'+n'')D_B}{r+1}=187{,}860\\ 1-\frac{\delta'}{g+\rho}=1\cdot00117\\ \text{Product}=V=188{,}079\end{array}\right\}\text{Correction for second term}=+219$$

The following is a summary of these results :—

12th and 13th equalities.

1880 December 21, Nos.	1 and 2	V=187,707	miles per second.
„ „	5 „ 6	188,405	„
„ „	8 „ 9	187,676	„
„ „	9 „ 10	186,457	„
„ „	10 „ 11	185,788	„
1881 January 20, Nos.	3 „ 4	186,495	„
„ „	5 „ 6	187,003	„
Mean for 12th and 13th equalities:		V=187,076	„

13th and 14th equalities.

1880 December 21, Nos.	2 and 3	V=186,190	miles per second.
1881 January 20, Nos.	6 „ 7	186,830	„
„ „	7 „ 8	187,266	„
„ January 21, Nos.	2 „ 3	188,110	„
„ „	3 „ 4	188,079	„
Mean for 13th and 14th equalities:		V=187,295	„
General mean of both sets . .		V=187,167	„

Multiplying this by the mean refractive index of air (=1·00029) we obtain the value for the velocity in vacuo, viz.: 187,221 miles per second.

This must be corrected for the rate of our clock.

One second of our clock is equal to 0·999723 of a mean solar second.

Dividing the value found for V by this quantity, we obtain the final value for the velocity of the white light from an electric lamp in vacuo, viz. :—

V=187,273 miles per second (log=5·2724757)
=301,382 kiloms. per second (log=5·4791167)
Using Struve's constant of aberration 20″·445.
The resulting parallax of the sun is =8″·77.
Distance of the sun =93,223,000 miles.

The value obtained by Cornu,[*] using the method of Fizeau, was 300,400 kiloms. per second. He nearly always used the Drummond (or lime) light. A few experiments were made with a petroleum lamp.

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[*] "Annales de l'Observatoire de Paris" (Mémoires, tome xiii.), 1876

The value obtained by MICHELSON,[*] using a modification of the method of FOUCAULT, was 299,940 kiloms. per second. He always used the light of the sun when near the horizon (in the early morning or late afternoon), except in a single set of observations where he used the electric light, and which he considered unsatisfactory. Thus we have three series of very carefully conducted experiments to determine the velocity of light, each one differing essentially from the others in their method of research; the results are all very close to each other, and we believe that in the sequel we shall be able to show reasons for the outstanding differences. Grouping the three sets in order we have:—

	Usual source of light.	Method.	Results for V.
			Kiloms.
MICHELSON[*] . . .	The sun near horizon .	Deflection by mirror	299,940
CORNU[†]	Lime light	Toothed wheel and eclipses .	300,400
YOUNG and FORBES .	Electric light . . .	Toothed wheel and equalities .	301,382

CORNU—MICHELSON	460	kiloms.	per second.
YOUNG and FORBES—MICHELSON.	1,442	„	„
YOUNG and FORBES—CORNU . .	982	„	„

After we had completed the observations which have now been reduced, we found reason for believing that the velocity of light depends upon its colour, and further examination of the question confirmed us in this opinion. It seemed useless then to continue to measure the velocity of a light whose colour changes considerably and quite sufficiently to give us values for the velocity of light varying much more than any errors of observation could make them vary. We then devoted our attention to an examination of the question involved in the second part of our research, viz.: whether the velocity of light depends upon its colour, and if so to what degree.

PART II.

RELATIVE VELOCITY OF LIGHTS OF DIFFERENT COLOURS.

Does the velocity of light depend upon its colour?

Before describing the observations which furnish an answer to this question, we will briefly recapitulate the general arrangements in our method of working, so that those who wish it may study this part of our research independently of the rest.

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[*] "Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac." Vol. i., part iii., 1880.

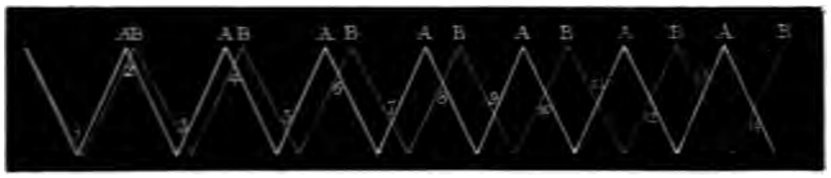
[†] "Annales de l'Observatoire de Paris" (Mémoires, tome xiii.), 1876.

Our method is very like that of M. Fizeau. We have an observing telescope, in the focus of which, and in a plane perpendicular to the axis of which, we have a toothed wheel revolving with very great velocity. The light from the sun or from an electric or other lamp is condensed by a lens, and reflected by a diagonal reflector in the eye-piece, so as to throw an image of the incandescent carbons upon the toothed wheel. The light passing between the teeth goes to a distant reflector and returns by reflection to the object-glass of the observing telescope, by which the rays are brought to a focus in the plane of the toothed wheel at that exact part of the focal plane whence the rays had emerged which were capable of striking the distant reflector. This point in the focal plane is of course that point at which the observer sees the distant reflector (through a small aperture in the diagonal reflector), or at which he sees a star of light when the lamp is in action.

If the double distance to the distant reflector and back were about six miles, we know, roughly speaking, light takes about $\frac{1}{31000}$ second to traverse the double distance. Now suppose the wheel to rotate very slowly. We see alternately a tooth of the wheel, and a star of light shining in the interval between two teeth. If the speed be increased so that more than 10 teeth pass in a second, the persistence of visual impressions causes us to be unable to distinguish these alternate phases. We see a star of light continuously upon a partially illuminated field. If in $\frac{1}{31000}$ second a tooth passes into the position previously occupied by a space, then the light which passed away through a space to the distant reflector is on its return stopped by a tooth and we see nothing but the tooth, while when a space between two teeth is at that part of the field of view where the star should appear no star of light is seen, because $\frac{1}{31000}$ second ago a tooth occupied that position, and no light could get through to go to the distant reflector. If there be 400 teeth in the wheel this speed of revolution is $\frac{800}{31000}$ of a second to one revolution, or 38·75 revolutions a second. At this speed of revolution no star of light would be seen, but if the speed be doubled the light passing out through a space to the distant reflector can on its return pass through the next space to the eye of the observer, and the light is seen with its full intensity. If the speed 38·75 revolutions a second be increased threefold, fivefold, &c., or any odd number of times, we have an eclipse of the star. If that speed be increased twofold, fourfold, &c., or any even number of times, we have full brightness.

Fizeau, and Cornu after him, measured the speed required to produce an eclipse, and thence they deduced a value for the velocity of light. Our method, however, differs distinctly from theirs in this way: that in place of having a single reflector in the distance, we have two at different distances from the observing telescope, but nearly in the same line with it, so that the observer in looking through the telescope sees two stars side by side separated from each other by a distance of about 25″ of arc. While the toothed wheel is being rotated with gradually increasing speed, the star coming from the more distant reflector (which we call A) is eclipsed before that one coming from the nearer reflector (which we call B). As the speed increases A grows brighter, while B is still diminishing in brightness, and at a certain speed they are of equal brightness. In the same way in the different phases, as the speed of the wheel is increased more and more, we have a succession of equalities in the two stars. The distance of A from the observer is to that of B in the ratio of about 13 to 12. Hence it follows that the sixth maximum of B coincides with the seventh minimum of A. In the following figure abscissæ represent speeds of revolution of the toothed wheel, and ordinates intensities of the stars. The intersection of the two lines indicates equality of brightness and shows the speed required to produce it.

Fig. 2.



The lines in the above figure which indicate the brightness are subject to certain alterations dependant upon the necessary imperfections in the optical and mechanical parts of the apparatus. It has been shown in the previous part of this memoir that our method is in general unaffected by these alterations. Most of our observations have been made at speeds corresponding to the 12th, 13th, and 14th equalities, and it will be unnecessary in the diagrams which represent further development of the theory to delineate other parts of the diagram.

Distinctive colours observed in the return light.

Having made these preliminary remarks, we will now proceed to trace the steps by means of which the relation between colour and velocity has been suspected, and the quantity of the effect has been approximately determined.

In the course of our observations made with sunlight at Pitlochry in 1878, and in those made with the electric light at Kelly in 1880–81, we were frequently annoyed by the presence of colour in the stars, one of them appearing reddish and the other bluish. This made it very difficult to appreciate the exact speed which might be said to produce equality in the lights; for, as is well known, it is very difficult, if not impossible, to judge accurately of the equality of two lights of different colours. We considered that these colours arose from a want of accurate adjustment of the distant reflectors. These consist each of a telescope tube with an achromatic object-glass at one end, but with no eye-piece, and having a silver mirror at the focus of the object-glass. An image of the object-glass of the observing telescope is thrown by the rays from the source of light upon the silver mirror, whence the light is reflected back to the observing telescope. Now the quantity of light which is reflected back *into* the observing telescope depends largely upon the accuracy of focus of the reflecting collimator. If, then, the object-glass be not accurately achromatised the one reflector may be focussed accurately for blue rays and the other for red rays. Thus we should have one star intrinsically redder than the other. In consequence of these considerations, we used, whenever we noticed a difference in colour, to mention the fact, in order that this cause of inaccuracy in the observation might be taken account of. We did not take particular notice which star was red and which blue, though we sometimes noted the fact. We had no idea that any information might be gained by always noticing which star was red and which one blue.

On different days the distinctness and steadiness of the stars varied enormously. The days when the stars were steady and distinct were the days on which we got the best observations, and felt most certainty about the exact speeds which produced equality in the two stars, except that it was often on these days that the difference in colour troubled us most.

On the 11th February, 1881, we were making the regular observations for determining the speed of revolution of the toothed wheel required to give the 12th, 13th, and 14th equalities, corresponding to speeds of about 410, 450, and 490 revolutions a second. These speeds were obtained by using three, four, or five weights to drive the mechanism. The observations in the observing book are numbered from 1 upwards. The following observations, 1 to 5, were taken between 9.30 A.M. and 9.50 A.M. The remarks are extracted from the observing book, and were written at the time, an entry being made after each observation.

"*February* 11, 1881.—Splendid morning. A and B very bright and steady. If anything, A is greater than B.

1.—Three weights. B increasing with increase of speed. B reddish, A bluish (?).
2.—Four weights. A increasing „ „ „ A „ B „
3.—Five weights. B increasing „ „ „ B „ A „
4.—Four weights. A increasing „ „ „ A „ B „
5.—Three weights. B increasing „ „ „ B „ A „
(Not a very good observation.)"

The mark (?) expressing doubt about the colour in the first observation was inserted after the second observation had been made, and was so inserted because they seemed to be antagonistic.

When the observations numbered 6 and 7 respectively were being taken at 10.55 A.M., the following remark is entered in the observing book: "The same phenomenon as above." This refers to the colours.

A number of trials were then made at different speeds (in which no use was made of the chronograph) to examine still further this remarkable phenomenon, and the following statement is made in the observing book:—"Always the light which is increasing with respect to the other, with increase of velocity [of the toothed wheel], appears red; and the other one blue." The words in square brackets are not in the original, and are inserted here to make the statement clearer.

These observations clearly proved to us that the colour which we had often observed was not always due to the adjustment of the distant reflectors. For here sometimes the one and sometimes the other was the red one. At each successive equality (*e.g.*, the 11th and 12th, the 12th and 13th, &c.) the colours of A and B are reversed.

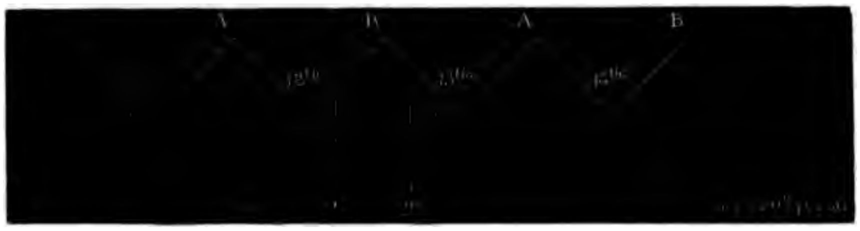
Since February 11 there certainly have been many days when the colour-differences were not perceptible. It may perhaps have been because the stars were not steady or were flickering or indistinct. On these occasions the atmospheric refraction disturbs the course of the rays, so that the teeth of the wheel being extremely minute, a ray of light which, if there were no irregular atmospheric refraction, would not reach the reflector, does so under these circumstances. In such a case the stars do not alter their intensities, with change of speed of the toothed wheel, so regularly as they do when the atmosphere is not unequally heated and disturbed.

The general result however was established by the observations on February 11, 1881, but it is not a common observation.

Explanation of the colours perceived in the return light.

The simplest explanation which can be given of these phenomena, and the only explanation which seems to be capable of standing all kinds of tests, is that *the different colours travel with different velocities, the more refrangible rays, or those with shortest wave-length, travelling quickest.* If this were the case we should be forced to alter our diagram indicating the intensities of A and B. We should have as many curves of intensity for each of the two stars of light as there are colours in the light we are employing. Let us examine only two of these colours (red and blue for example). If the red light travel slower than the blue a smaller velocity is required to produce an eclipse with red light and with blue. For this reason the curve representing intensity in terms of speed of rotation for red light will have its maxima and minima lagging gradually more and more behind those for the blue light. We are in general dealing only with the speeds of rotation which produce the 12th, 13th, and 14th equalities; and during that small variation in speed the lines for red and for blue light may, for purposes of illustration, be drawn sensibly parallel. The curves for the two stars A and B would then be shown approximately by the following diagram, in which dotted lines represent red light, and full lines blue light. Here we notice that at x the light of A is diminishing with increase of speed, and the abscissa corresponding to blue light is greater than that corresponding to red light. Hence, *when the intensity is diminishing with increase of speed the star should have a blue tinge.* But at y the light of A is increasing with increase of speed, and the abscissa corresponding to red light is greater than that corresponding to blue light. Hence, *when the* *intensity is increasing with increase of speed the star should have a red tinge.* Observation confirms these statements. Hence the observations can be explained on the assumption that blue light travels quicker than red light.

Fig. 3.



The analytical expression of this result is quite simple. §§ 2 and 3 of the mathematical theory give the following values for the intensities of red and violet light (indicated by the suffixes R and V):

$$\text{1. Light increasing with increase of speed}\begin{cases} I_R=\frac{E}{2}\left\{2(1-\kappa)-p+\frac{n}{N_R}\right\} \\ I_V=\frac{E}{2}\left\{2(1-\kappa)-p+\frac{n}{N_V}\right\} \end{cases}$$

But if the velocity for violet is greater than for red light, N_V is greater than N_R, and hence I_R is greater than I_V in the even phases, and the return light will appear tinged with red.

$$\text{2. Light diminishing with increase of speed}\begin{cases} I_R=\frac{E}{2}\left\{2(1-\kappa)+p-1-\frac{n}{N_R}\right\} \\ I_V=\frac{E}{2}\left\{2(1-\kappa)+p-1-\frac{n}{N_V}\right\} \end{cases}$$

Here, on the other hand, if N_V be greater than N_R, we have I_V greater than I_R, and the return light will be tinged with blue, in the odd phases.

First measurements of the difference in velocity of red and white light.

While we were quite prepared to examine every possible source of error in these new and unexpected conclusions, we considered it to be of first importance to attempt to get, even in a rough manner, some actual measurements of the difference in velocity of red and blue light, on the assumption that such a difference is the explanation of our results. From the above figure it will be seen that the speed of rotation necessary to give equality of lights must always be greater for blue than for red light. It is also clear that the difference in speed of rotation for red and for blue light bears the same relation to the absolute speed of rotation for either of those colours as the difference in velocity between rays of red and blue light bears to the absolute velocity of that colour.* The same thing will hold, though to a less extent, on comparing red and white light. Our only means at hand on February 11 was to determine the speed which produced an equality (1) in the ordinary way with the white light of the electric lamp, and (2) with the eye screened by a piece of ruby red glass.

The observations made with this object on February 11 are named in the observing book No. 13 (white), No. 13 (red), No. 14 (white), and No. 14 (red).

They were not very satisfactory, for the differences found between the velocities of red and white light were small. The observations No. 13 were at the speed producing the 13th equality. The observations No. 14 were at the speed producing the 14th equality. The speeds of rotation finally deduced from the chronograph records were as follows :—†

		Difference.
Observation No. 13 (red) speed of rotation	$456^{r}{\cdot}84$	$4^{r}{\cdot}14$
,, No. 13 (white) ,, ,, ...	$460^{r}{\cdot}98$	
,, No. 14 (red) speed of rotation	$494^{r}{\cdot}85$	$1^{r}{\cdot}57$
,, No. 14 (white) ,, ,, ...	$496^{r}{\cdot}42$	

$$\frac{\text{Difference of velocity (red and white)}}{\text{Absolute velocity (white)}}=\begin{cases}\cdot90 \text{ per cent. from No. 13}\\ \cdot32 \text{ ,, ,, No. 14}\end{cases}$$

These differences are small; but on the whole they indicate a greater speed for white than for red light. But these differences might be suspected to be due to irregularities in the action of the chronograph. The general result seemed to be that we must obtain a greater difference in speed by choosing two colours of light, differing considerably in wave-length, and that we might with advantage discard the chronograph as an absolute measurer of speeds, and adopt some more delicate means of measuring minute differences of speed.

Great difficulty was found in obtaining a blue medium which would sufficiently stop out the red rays. The ordinary blue glass, coloured with cobalt, allows large quantities of red light to pass. We tried eight or nine solutions, which we put into glass cells with parallel sides and tested with a prism. We found that a nitrate of copper solution gave the least quantity of red.

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* § 4 of the mathematical theory gives us the value of n for the r^{th} equality for red light, viz.,

$$n_{\text{R}}=\frac{\{2(1-\kappa)(1-\rho)+r(1+\rho)\}\text{N}_{\text{A}}\text{N}_{\text{B}}}{\text{N}_{\text{B}}+\rho\text{N}_{\text{A}}}=\kappa.\frac{\text{N}_{\text{A}}\text{N}_{\text{R}}}{\text{N}_{\text{B}}+\rho\text{N}_{\text{A}}}$$

If now $\sigma=\frac{\text{velocity of violet light}}{\text{velocity of red light}}$ we notice that for violet light N_{A} and N_{B} must be multiplied by σ.

Thus we have

$$n_{\text{V}}=\kappa.\sigma.\frac{\text{N}_{\text{A}}\text{N}_{\text{B}}}{\text{N}_{\text{B}}+\rho\text{N}_{\text{A}}}=\sigma.n_{\text{R}}$$

$$\frac{n_{\text{V}}}{n_{\text{R}}}=\sigma$$

also $\frac{n_{\text{V}}-n_{\text{R}}}{n_{\text{R}}}=\frac{\text{difference of velocities for red and violet}}{\text{velocity of red light}}$ Q. E. D.

† The observations and reductions are in the hands of the Royal Society.

First differential observations for red and blue light.

After February 11 we never had a clear day or night to continue our research until the 21st of the month. We then made observations in the following manner:—A thick piece of indiarubber tubing was attached to the top of the pulley which supports the weights driving the toothed wheel. At its upper end it was attached to a string, which, passing over a fixed pulley, was held by the observer. The observer adjusted the driving weights so as to be a little in excess of what was required to produce equality of the lights. He then fixed the string tightly, and as the weights descended, the indiarubber was stretched, and diminished the effective driving weight. In this way a beautifully gradual diminution of velocity was obtained, accompanied by as beautifully gradual an increase and decrease in the brightness of the two stars respectively. Our plan was to place the blue solution between the lamp and the diagonal reflector. When equality of lights was attained the observer said "Stop" to the assistant, who then commenced to count seconds on the clock. At the same time the blue solution was replaced by a piece of ruby glass. When equality of red lights was attained the observer again said "Stop." The interval of time was then noted in the observing book. Our intention was to measure by means of the chronograph, at our leisure, the diminution in velocity produced by the action of the indiarubber during a given number of seconds.

Alongside of each observation we entered in the observing book the weights we used. But in the observations of February 21 these were nearly always the same, and it was always the 12th equality which was observed, being a speed of about 400 revolutions a second. It will nevertheless be well here to tabulate the names of the weights employed and their absolute weights, as reference is frequently made in the observing book to them. We usually had one large lead weight to begin with, and this was the one used when one lead weight is spoken of. It weighed 56 lbs. We had two other lead weights which might be added, each weighing 12 lbs. 2 oz. We had five iron weights, each weighing 10 lbs. 7 oz. These were generally supported by an additional iron hook weighing 1 lb. 11 oz. Smaller weights, two of which were called *a* and *b*, each weighing about 1 lb., and others weighing a few ounces, were used as well. The length of the indiarubber tubing which we generally, if not always, used was 10½ inches when unstretched. It lengthens 3 inches with the addition of 1 lb. weight.

With one lead weight (56 lbs.) the fall of the weight when driving the mechanism is 1 inch in 7 seconds.

With three lead weights, one iron weight and hook (92 lbs. 7 oz.), the fall is 1 inch in 5·3 seconds.

[*N.B.*—It must be remembered that the effective weight is half of the actual weight owing to the action of the pulley.]

On February 21 eight observations were made in the manner described above. A little practice was required to get accustomed to the method of observation, but after a few trials the results were wonderfully accordant. The following extract from the observing book contains all the observations made upon that day :—

"*February* 21.—No. 1. At 5.30 P.M. Two lead weights and *a*. Blue to red—interval =28 seconds."

The following were between 7 and 8 P.M.:—

"No. 2. Two lead weights and *a* + hook. Interval =9½ seconds.
"No. 3. Weights as before . . . . Interval =20 seconds.
"No. 4. " " . . . . Interval =24 seconds.
"No. 5. " " +*b*. Interval =45 seconds.
"No. 6. " " . . . . Interval =20½ seconds. Very good.
"No. 7. " " . . . . Interval =20 seconds.
"No. 8. " " . . . . Interval =21 seconds."

The following remark is added :—

"During the whole of to-day—while trying different methods to detect the difference in velocity of different coloured rays—I have at every step been struck by the enormous difference that exists. It is easy to get a velocity for which A is greater for blue light while B is greater for red light. Sometimes, when A and B are equal for blue, A or B is almost invisible for red, and the other at [near ?] its maximum."

On the evening of February 21 chronograph measurements were made to determine the loss of speed after the indiarubber had been in action for some definite interval of time. The interval adopted was about 18 seconds. The observations and reductions are in the hands of the Royal Society. The results of four such determinations are as follows :—

	No. 1.	No. 2.	No. 3.	No. 4.
Revolutions per second—				
At beginning of observation . .	487^{r}·22	514^{r}·77	502^{r}·33	502^{r}·33
At end " " . .	480^{r}·28	499^{r}·64	491^{r}·45	496^{r}·60
Diminution of speed in 18 seconds . .	6^{r}·94	13^{r}·13	10^{r}·88	4^{r}·73
" " 1 second . .	0^{r}·38	0^{r}·73	0^{r}·60	0^{r}·26

Average . . . . =0·49 revolution per second.

Although these measurements vary a little amongst each other, they give us sufficiently well a rough knowledge of the rate at which the indiarubber reduces the speed.

The average interval of time between the equality of red and blue lights was 23·5 seconds. This multiplied by 0·49 gives us a difference of 11·5 revolutions a second, or about 2·82 per cent. of the speed producing equality of white light (410 revolutions per second).

It appears then from these experiments that the difference in velocity of red and blue lights is about 2·82 per cent. of the total velocity of light.

The eight separate observations give the following values: 3·36, 1·14, 2·40, 2·88, 5·40, 2·46, 2·40, 2·52.

On February 23 a red solution was substituted for the ruby red glass, as it allowed fewer of the more refrangible rays to pass. In these experiments the chronograph was used directly; the velocity of rotation of the toothed wheel being reduced as before by means of the indiarubber tubing.

The observation book contains the following entry :—

"Lights very bright. A greater than B decidedly. Observations perfectly satisfactory. Interval between first and second signals [those for blue and red lights] about 20 seconds. Whole set [four observations] completed in 10 minutes. Atmosphere very clear all the time.

"No. 1. A increasing with increase of speed. Blue light. (When blue lights were equal, on changing to red light, A was far too bright.)

"No. 2. A increasing with increase of speed. Red light.
"No. 3. B „ „ „ „ Blue light.
"No. 4. B „ „ „ „ Red light."

The chronograph was used this day in a different manner to that usually adopted. Contrary to our expectation, the result was not satisfactory, and we do not think that the speeds obtained can be relied upon. We can only say therefore to-day that the general effect was the same as before. The defect we allude to is probably due to a small mistake in counting the number of pen marks in a certain space upon the chronograph cylinder. The readings of the chronograph and the reductions are, however, in the hands of the Royal Society.

Exceptional observation.

On February 24, 10 P.M., the following entry was made :—"Tried two speeds. (1) A increasing with increase of speed, (2) B increasing. Used red and blue lights. There seemed to be a decided tendency for *red* to require a *greater* velocity than *blue*, to produce equality. This is contrary to all our previous experience. A was decidedly brighter than B, but both were tolerably steady and bright."

A few minutes later the following observation was made :—

"With white light.

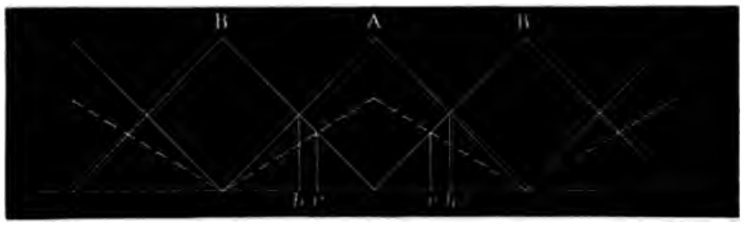
"A blue, B red. Increase of speed increases brightness of A. [This is contrary to previous experience.] This was with speed about 440.

"A blue, B red. Increase of speed increases brightness of B. [This is in accordance with previous experience.] This was with speed about 400.

"It seems then that to-night A is intrinsically bluer than B, owing probably to adjustment of focus. But will this explain all?"

Let us see now what would be the effect of A being intrinsically bluer than B. The following figure represents such a case on the supposition that blue and red light travel with the same velocity, and that A is deficient in red light. Full lines represent blue light, and dotted lines red. These coincide for B.

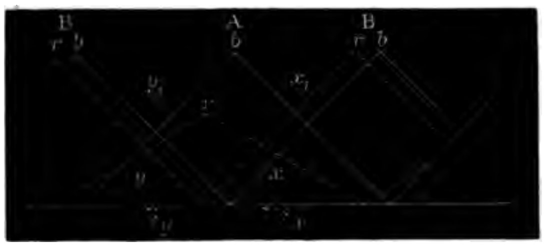
Fig. 4.



The result is clear that at different successive phases the blue and red rays alternately require a greater speed of rotation to produce equality of lights. But this does not in the least represent the results of our observations.

The following diagram illustrates the case where A is deficient in red light, on the assumption that blue rays travel quickest.

Fig. 5.



The maxima of A and B with different speeds are indicated by the letters A, B. The red is shown to reach its maximum before the blue. The red in A is shown to have a smaller maximum than the red in B. The intersections of the full and dotted lines represent the speeds producing equality of the blue and red rays respectively. Here δy is almost nil, whereas δx is too great. Hence, if we observe only at one speed, we may get far too great or far too small a difference between the velocities of red and blue light.

The mean of δy and δx is (nearly) that due to the difference in velocity of red and blue rays.

[*N.B.*—Our observations on February 23 were taken at the 12th and at the 13th equalities, so that this cause of uncertainty is eliminated.]

On February 25, 1881, 4 P.M., B was greater than A. A was bluish compared with B when the toothed wheel was removed. Three observations were made, but they show no decided difference between δx and δy. The indiarubber was used as before.

No. 1. 12th equality of lights . . . . . Blue-red = 2·5 seconds.
No. 2. 13th „ „ . . . . . „ „ = 4·0 „
No. 3. (at 5·30 P.M.) 12th equality of light . „ „ = 6·0 „

Now the loss of speed caused by the indiarubber in one second is 0·49 revolution per second. The 12th equality is at a speed of about 420 revolutions a second; and the 13th equality about 460 revolutions a second. From these we get the difference between the velocity for red and for blue light, in percentages of the whole, as follows:—

From No. 1 we have 0·29 per cent.
„ No. 2 „ 0·43 „
„ No. 3 „ 0·70 „

On February 27, between noon and 1 P.M., measurements were made in the usual way with the indiarubber, which was a most convenient method of working at different speeds:—

No. 1. 12th equality . . . . Blue-red = 11 seconds.
No. 2. 13th „ . . . . „ „ = 11½ „

(In this observation the indiarubber did not seem to act well after the blue equality).

The observations were continued at 4 P.M. at various speeds and slower than those we had previously experimented upon:—

No. 3. 9th equality . . . . Blue-red = 9·0 seconds.
No. 4. 10th „ . . . . „ „ = 15·5 „
No. 5. 9th „ . . . . „ „ = 16·5 „
No. 6. 10th „ . . . . „ „ = 11·0 „
No. 7. 10th „ . . . . „ „ = 14·0 „
No. 8. 11th „ . . . . „ „ = 9·5 „

The following remarks are appended:—

"Observations from 3 to 8, lights splendidly steady and equal. The difference in velocity for red and blue most striking. A and B are precisely the same colour. Both A and B looked like clearly defined circles with no diffraction phenomena. This is very uncommon, and the observations are therefore valuable."

On the same day some more chronographic measurements were made to determine the reduction in speed per second of time produced by the indiarubber, at the speeds of the 10th and 11th equalities. The reductions are in the hands of the Royal Society, and they gave us—

10th equality . .	0·34 revolutions per second	diminution	in 1 second.
11th „ . .	0·29 „	„	„
9th „ . .	0·31 „	„	„

Reducing the first and second observations as before, allowing for a diminution of speed of 0·49 revolution a second, per second—

$$\text{From No. 1 we have } \frac{\cdot 49\times 11\times 100}{420}=1\cdot 28 \text{ per cent.}$$

$$\text{„ No. 2 „ } \frac{\cdot 49\times 11\cdot 5\times 100}{460}=1\cdot 22 \text{ „}$$

Chronograph records were afterwards taken at about the speeds of the 9th, 10th, and 11th equalities, and the speeds read off at intervals of about 8 or 10 seconds. Thus we obtained the percentage loss of speed in one second. Reducing the observations of this day we have—

No. 3.	9th equality;	$\frac{\text{difference of velocity (blue and red)}}{\text{total velocity}}$	$=1\cdot 71$	per cent.
No. 4.	10th „	„ „	$=1\cdot 55$	„
No. 5.	9th „	„ „	$=3\cdot 14$	„
No 6.	10th „	„ „	$=1\cdot 10$	„
No. 7.	10th „	„ „	$=1\cdot 40$	„
No. 8.	11th „	„ „	$=0\cdot 68$	„

Mean of the day's observations $=1\cdot 51$

March 1, 1881.—In the interval between February 27 and March 1 we removed the wedge under the toothed wheel and brought the mechanism into an upright position. Seeing that the solutions which we had hitherto used to produce our colours allowed colours of very different refrangibilities to pass, we thought it would be satisfactory to try the effect of pure prismatic colours. To do this we mounted a bisulphide of carbon bottle-prism in front of the condenser and succeeded in throwing a pure spectrum upon the toothed wheel. The distance traversed by the rays from the prism to the toothed wheel was 24½ inches. The electric lamp was mounted upon rollers so that we could easily move it to change the colours. A motion of only about ½-inch of the spectrum changed the colour of the stars from blue to red. This was a very convenient and pleasant method of working. The indiarubber was used as before. The following remarks are extracted from the observing book:—

"Reduced velocity by stretched indiarubber. Used prism for colours. Deflection of spectrum in inches given below [this was measured approximately by our assistant].

" No. 1.	Green to orange. . .	$\frac{1}{2}$-inch,	12·5 seconds,	12th equality,	good
" No. 2.	Green to reddish-orange	$\frac{5}{8}$-inch,	15 ,,	13th ,,	good
" No. 3.	Blue to orange . . .	$\frac{7}{16}$-inch,	9 ,,	14th ,,	fair
" No. 4.	Deep blue to blood-red	$\frac{5}{8}$-inch,	9 ,,	14th ,,	very good.

" This was a most successful mode of observation. Though B was much greater than A the equality was well determined each time."

In reducing these observations it is clearly impossible to take into account the exact refrangibilities or wave-lengths of the colours named. The general change was from a slightly greenish-blue to a red tinged with orange. Reducing as before, we have—

No. 1. $\frac{\text{Difference in velocity (blue and red)}}{\text{total velocity}} = 1{\cdot}35$ per cent.
No. 2. ,, ,, $= 1{\cdot}56$,,
No. 3. ,, ,, $= 0{\cdot}90$,,
No. 4. ,, ,, $= 0{\cdot}90$,,

March 8, 1 P.M.—It might, perhaps, be thought that we had now thoroughly tested and confirmed our first conclusions by the variety of our tests. But we were anxious to leave no room for doubt and to vary the tests in every possible way, and we thought that some advantage might be gained by changing entirely our method of altering the speed, so as gradually to increase it, the reverse of what we had done with the indiarubber. Accordingly we attached an iron crucible to the weights and led a thick indiarubber tube from it up to a funnel with a stop-cock, in reach of the observer. We filled this with mercury, and having put on such weights as were barely sufficient to produce equality, we turned on red light (using the prism method) and opened the stop-cock. So soon as equality of red lights was attained, we said " Stop " to the assistant, who then commenced to count seconds on the clock. Meantime he had instantaneously changed the colour of the light to blue. When the blue equality arrived we again said " Stop." He gave us the interval in seconds, and the deflection of the spectrum in fractions of an inch, which we entered in the observing book. Chronograph tests were afterwards made to measure the increase in speed produced in a second by the flow of mercury. This method seemed to us to be hardly of so great delicacy as the indiarubber method, but we felt much interest in seeing whether so entirely a different method could give results approximating to those obtained in the previous researches.

The following remarks are extracted from the observing book :—

" *March* 8, 1 P.M.—No. 1. Blue-red =16 seconds. Exact time uncertain to a few seconds. General effect positive " [*i.e.*, the blue equality required a higher speed].

"After blue equality turned again to red, and B was much brighter" [than A], "B increasing with increase of speed. Spectrum deflection about $\frac{5}{8}$ inch." [This was the 12th equality.]

"No. 2. Ditto. Same in every way. Blue-red $=9$ seconds. $\frac{5}{8}$ inch deflection.

"No. 3. Same arrangements. Blue-red $=5$ seconds nearly. Deflection $=\frac{3}{4}$ inch.

"Want of oil in (1), (2), and (3) made velocity irregular. We tried one more observation, but the motion of the wheel seemed quite irregular, and though it gave the same value (16 seconds) as No. 1, we could not trust it, because on turning on red light again A was brightest. We now oiled the mechanism with great effect in steadying the motion and diminishing noise. But the lights had begun to flicker and we got no good observations."

At 4 P.M. the lights improved.

"4 P.M.—Oiled axles.

"No. 4. 12th equality. Blue-red $=8$ seconds. Deflection $=\frac{9}{16}$ inch; but it did not look so great a change of colour as usual.

"No. 5. Same arrangements. Blue-red $=10{\cdot}5$ seconds. Deflection $=\frac{11}{16}$ inch. (The change in colour looked greater.) After equality of blue, tried red again, B was far brightest.

"No. 6. Same arrangements. Blue-red $=6$ seconds. Deflection $=\frac{11}{16}$ inch.

"No. 7. 11th equality. Blue-red $=7$ seconds. Deflection $=\frac{5}{8}$ inch.

"No. 8. Ditto. Blue-red $=6{\cdot}5$ seconds. Deflection $=\frac{5}{8}$ inch."

"8.45 P.M.—No. 9. Same conditions as No. 1" [12th equality]. "Blue-red $=16$ seconds. Deflection $=\frac{5}{8}$ inch.

"No. 10. Same conditions. Blue-red $=23$ seconds. Deflection $=\frac{5}{8}$ inch. Speed varied very slowly.

"Oiled the mechanism.

"No. 11. Blue-red $=10$ seconds. Deflection $=\frac{1}{2}$ inch [12th equality].

"No. 12. Blue-red $=9{\cdot}5$ seconds. Deflection $=\frac{5}{8}$ inch [12th equality].

"No. 13. Blue-red $=9$ seconds. Deflection $=\frac{3}{4}$ inch [11th equality].

"No. 14. Blue-red $=7$ seconds (good). Deflection $=\frac{3}{4}$ inch [11th equality]."

Reducing these results with the aid of the chronograph tests for the mercury method, we obtain the following 14 percentages for the values for

$$\frac{\text{difference of velocity of blue and red light}}{\text{mean velocity of light}}:\text{—}$$

(1) 3·20
(2) 1·80
(3) 1·00
(4) 1·60
(5) 2·10
(6) 1·20
(7) 1·40
(8) 1·30
(9) 3·20
(10) 4·60
(11) 2·00
(12) 1·90
(13) 1·80
(14) 1·40

The mean of these is 2·03 per cent., giving a result which confirms in a satisfactory manner the results arrived at by the other methods.

April 26 and 27.—After an absence of a month from Kelly we again tested the colour effect on April 26 and 27. We used wheels with 250 teeth and with 400 teeth, bright and smoked, upright and inclined. We ascertained the fact that blue light required the greater velocity of rotation to produce equality; but we did not make any actual measurements. We were unable to detect the successive phases of colour which were observed on February 11.

By collecting results of all the observations on the effect of colour on velocity we shall be able to judge of the general effect:—

February 11.—The star which was increasing with increase of speed was red; the star which was waning with increase of speed was blue.

The percentage difference of red and white light, two observations, 0·90 and 0·32.

February 21.—Ruby-red glass and blue copper solution. Speed diminished gradually by indiarubber. Eight observations—

(1) 3·36
(2) 1·14
(3) 2·40
(4) 2·88
(5) 5·40
(6) 2·46
(7) 2·40
(8) 2·52

Mean percentage difference = 2·82.

February 23.—Red solution and blue solution. Direct measurements of the change of velocity. Two observations at different speeds—

(1) 1·96
(2) 0·36

Mean percentage difference = 1·16.

February 24.—The effect seemed to be of the opposite kind to that hitherto obtained.

February 25.—Red and blue solutions. Indiarubber method. Three observations—

(1) 0·29
(2) 0·43
(3) 0·70

Mean percentage difference = 0·47.

February 27.—Red and blue solutions. Indiarubber method. Eight observations—

(1) 1·28
(2) 1·22
(3) 1·71
(4) 1·55
(5) 3·14
(6) 1·10
(7) 1·40
(8) 0·68

Mean percentage difference = 1·51.

March 1.—Pure spectrum thrown by bisulphide of carbon prism upon the toothed wheel. Blue and red parts of the spectrum used in succession. Indiarubber method. Four observations—

(1) 1·35
(2) 1·56
(3) 0·90
(4) 0·90

Mean percentage difference = 1·17.

March 8.—Spectrum used for colour. Mercury used to increase speed gradually. Fourteen observations—

(1) 3·20 (4) 1·60 (7) 1·40 (10) 4·60 (13) 1·80
(2) 1·80 (5) 2·10 (8) 1·30 (11) 2·00 (14) 1·40
(3) 1·00 (6) 1·20 (9) 3·20 (12) 1·90

Mean percentage =2·03.

Tabular statement of percentage differences in the velocity of red and of blue light.

February 21.	February 23.	February 24.	February 25.	February 27.	March 1.	March 8.	April 26.	April 27.
+3·36	Positive effect.	— . . .	+0·29	+1·28	+1·35	+3·20	Positive effect.	Positive effect.
1·14			0·43	1·22	1·56	1·80		
2·40			0·70	1·71	0·90	1·00		
2·88				1·55	0·90	1·60		
5·40				3·14		2·10		
2·46				1·10		1·20		
2·40				1·40		1·40		
2·52				0·68		1·30		
						3·20		
						4·60		
						2·00		
						1·90		
						1·80		
						1·40		
Mean +2·82	+ . . .	— . . .	+0·47	+1·51	+1·17	+2·03	+ . . .	+ . . .

General mean of 37 observations =1·8.

We cannot account for the apparently negative effect obtained on February 24. But when we consider that it was only for a short time that the observations were attempted, and that the appearance of a negative effect was extremely feeble (almost doubtful), and that in so delicate an observation the state of our health might affect the sensitiveness of our appreciation of colour effects, and that on other days the positive effect was most marked and indubitable, as recorded in numerous passages in our observing book, and quoted above,—when we consider all these things we cannot place this single dubious night's evidence in opposition to the overwhelming testimony of so many positive observations. It was too much to expect that we should be able in the first instance to measure, by the differential methods described above, the difference produced by a given difference of wave-length with very great precision. But there seems little room for doubt that the general mean of these 37 observations cannot be far from the truth, and we may affirm that when the wave-length changes from about $\lambda=50$ tenth-metres to about $\lambda=60$ tenth-metres, the velocity changes about 1·8 per cent., or in any case somewhat over 1 per cent.

Let us now consider some of the effects which must follow from these results.

Refraction and dispersion.—There is nothing in the undulatory theory of light, independent of our views as to the kind of force acting in the ether, which is opposed to the view that red and blue lights travel with different velocities in vacuo. We know that the ratios of the velocities of red or blue lights, in a refracting medium and in vacuo, are equal to their refractive indices. But the theory of refraction or dispersion tells us nothing about the ratio of the velocities of red and blue lights either in vacuo or in refracting media.

Interference and diffraction.—So with the phenomena of interference and diffraction, which give us a measure of the wave-length of the different colours. If we could measure the period of vibration of the different colours, this would show us the difference in velocity in the different colours. But our knowledge of the period of vibration is dependent on the velocity of light, and these phenomena give us no information upon the phenomena we are discussing.

Experiments of CORNU *and* MICHELSON.—Why did not these experimenters notice the effect of colour? First, consider the work of CORNU. We might expect that he should have seen colours in his star of light near the eclipses. But when we have a very feeble light changing its intensity it is very difficult to appreciate differences in colour, and he had no light for comparison as we had. He depended largely for the accuracy of his results on two facts: (1) that he took a mean of 546 pairs of observations, so reducing the probable error to one twenty-third part of that of a single pair of observations, and (2) that he used eclipses as high as the 21st eclipse, which reduces the personal error of the eye observation to one forty-first part. We may add that he had five phases (which he calls U, *u*, V, *v*, and *ν*) differing slightly, and he had considerable latitude in placing any single observation in one or other of these phases. We may also add that into each set of observations he introduced what he called "rectifications probables," which assisted to eliminate discordances.

If, however, we examine his non-rectified results, and classify them according to the source of light used, we shall, we believe, obtain confirmation of our views.

Neglecting the small correction depending upon the phase ($U+u$ or $V+v$) we have the following results:—

Source of light.	Number of observations.	Approximate velocity.
		Kiloms.
Petroleum	20	298,776
Sun near horizon . .	77	300,242
Lime light	449	300,290

Compare this with the absolute determinations given on p. 270.

	Usual source of light.	Velocity.
MICHELSON	Sun near horizon. .	299.940
CORNU	Lime light	300,400
YOUNG and FORBES .	Electric light . . .	301.382

In every case the more refrangible the mean colour of the source of light the greater is the velocity.

In MICHELSON'S observations the image of the slit was described as indistinct and covering a sensible space. From our results it would appear that the width of his spectrum between mean red and blue would be about 2 millims. But it would be a very impure spectrum, and it is only by employing absorptive media, or part of a pure spectrum, to give colour to the light used, that we should expect him to detect the difference.

Solar eclipses and occultations.—We might expect that the sun or a star on disappearing behind the moon would appear to be red, and on reappearing would first flash out blue. But light takes only 1½ second to come from the moon and the one-fiftieth or one-hundredth part of this is too small an interval of time for us to have any hopes in this direction.

Eclipses of Jupiter's satellites.—There must be a difference in time between the disappearance or reappearance of the blue or red parts of the light of a satellite, amounting to about half a minute. But the change is so gradual and the light when near eclipse so feeble that we fear this would be a very difficult determination.

Aberration.—A star exposed fully to the action of aberration must be drawn out into a spectrum parallel to the direction of the earth's motion. The length of the spectrum between the mean red and blue must be about $0''\cdot36$. It is possible that with a reflector of good definition this effect might be detected.

Temporary stars.—The mean light from the nearest star whose parallax is known takes 3½ years to reach the earth. The difference in time taken by the blue and red rays to reach us must be 13 days. The star T Corona flashed out suddenly in 1866. It was first seen on May 9, and there seems to be good evidence to show that it was not conspicuous a weak earlier. On May 9 it was of the second magnitude, and it diminished in brightness for some days at the rate of a magnitude a day. On the 12th May it was examined by Dr. HUGGINS with a spectroscope and showed bright lines in very different parts of the spectrum. This is somewhat contrary to what we should have expected. But we are not aware that the evidence for the identity of the temporary star and the small one known to be in that position is irresistible, nor do we know that the parallax of that small star has been very carefully studied, nor is the evidence as to the invisibility of the star at the beginning of the month conclusive.

Variable stars.—When a variable star brightens the first colour to reach us in increased intensity should be blue, and when it fades the last colour to cease shining in full splendour should be red. If we could determine the difference in time between the time of maximum for blue and for red rays, the present research would enable us to determine approximately the distance of the star from us.

Variable stars are certainly known to change colour; but the only reference to a systematic law in this connexion which we have ever come upon is in WEBB's 'Celestial Objects for Common Telescopes,' where (Edition II. (1866), p. 208) the following remarkable passage occurs:—

"HIND thinks several variable stars increase blue, are yellow after maximum, and flash red in decreasing."

This is in exact accordance with the results of the present research.

We have applied to Mr. HIND for details of his observations. He informs us that the remark quoted by WEBB was made in a letter to the "Times," some twenty-six years since, on the occasion of notifying a newly discovered variable. He does not, however, feel justified in now advising us to place reliance on this as a law.

It appears, then, that our conclusions as to the relation between colour and velocity must for the present rest upon the merit of our observations. We have ourselves no hesitation in saying that the effects were so striking and so decided on the greatest number of occasions, that we ourselves have no doubt as to the general conclusion. But we admit that much is still left to be done in the way of absolute determinations. There is little doubt that a further investigation into the matter would help to a further knowledge of the properties of the ether (if there be such a substance). This end would be specially aided by an exact determination of the law of dependence of velocity upon wave-length. We believe that, with certain improvements in our apparatus, in a better climate, this determination might be completed.*

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* The details of our observations on the dependence of velocity on colour are deposited with the Royal Society.

VI. *Action of Free Molecules on Radiant Heat, and its Conversion thereby into Sound.*

By JOHN TYNDALL, *F.R.S.*

Received January 23, 1882.*

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§. 1. *Introduction.*

THE experimental researches of RUMFORD and LESLIE raised the subject of Radiant Heat to an extraordinary pitch of interest and importance. Both of these philosophers occupied themselves with what may be called superficial emission and absorption.

MELLONI is to be regarded as the founder of our knowledge of the transmission of radiant heat through solids and liquids. Save in a passing inference, to be noticed immediately, MELLONI left untouched the gaseous form of matter; thinking, probably, that gases and vapours, though their diathermancy could hardly be supposed theoretically perfect, came in this respect so near perfection as to be placed beyond the grasp of laboratory experiment. It was doubtless the general prevalence of this conviction which caused this field of inquiry to lie fallow for so many years after the discovery of the thermo-electric pile.

By an experimental arrangement characteristic of the genius of the man, though, it may be, not quite equal to the requirements of the problem, MELLONI proved that the law of inverse squares held good for radiant heat *in air*, and from this he inferred the absence of all sensible absorption, by air, within the distance embraced by his experiments[*]. **Melloni** extended to radiation his conclusion regarding absorption. "On ne connait," he writes, "aucun fait qui démontre directement le pouvoir émissif des fluides élastiques purs et transparents."[†] Such was **Melloni**'s relation to the subject now before us.

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* The substance of this paper was delivered orally as the Bakerian Lecture on November 24, 1881.

In 1855 Dr. **Franz** of Berlin published a paper "On the Diathermancy of certain gases and coloured liquids."[‡] He found that air contained in tubes 452 and 900 millimeters long, absorbed 3·54 of the radiation from an **Argand** lamp, and he concluded that all transparent gases would behave like air. I have given reasons for holding that Dr. **Franz** in these experiments did not touch the question in hand.[§] In the arrangement which he describes, the absorption by air was quite insensible. But 60 per cent. of the radiation from his powerful source was lodged in the glass ends of his tubes; these, as secondary sources, radiated directly and indirectly against his pile, and it was their chilling by the cold air that slightly lowered his deflection and produced the supposed absorption.

It is not improbable that other attempts were made to bring gaseous matter under the dominion of experiment; but none to my knowledge are recorded.

§. 2. *Partial Summary of previous work.*

My researches on magne-crystallic action carried with them the incessant use of conceptions and reasonings touching molecular constitution and arrangement. At an early period of these studies it occurred to me that heat, both in its radiant and in its ordinary thermometric form, might be turned to good account as an explorer of molecular condition. The first fruit of this idea was a paper "On Molecular Influences,"[||] in which it was shown that wood possesses three axes of calorific conduction coincident with the axes of elasticity discovered by **Savart**. Experiments on certain crystals recorded in this paper suggested a possible connexion between diathermancy and conductivity, and in 1853 I worked at this question. The substances then submitted to experiment were rock crystal, amethyst, topaz, beryl, rock-salt, smoky quartz, fluor spar, tourmaline, Iceland spar, dichroite, arragonite, heavy spar, flint, and glass of various kinds. These minerals were employed in the shape of cubes carefully cut and polished, the transmission through each of them, in different directions, both of radiant and conducted heat being determined.

A desire for completeness, not then attained, caused me to postpone, and finally to forego the publication of the results of this inquiry. It, however, kept alive reflections on the influence of molecular constitution on the phenomena of radiation and absorption. Encountered continually by the thought that in liquids and solids the pure molecular action was, or might be, hampered by cohesion, the desire to bring, if possible, free molecules under the dominion of experiment beset me more and more.

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[*] "Pour un intervalle de cinq à six mètres, l'air n'exerce aucune absorption sensible pour le rayonnement des corps chauds," 'La Thermochrose,' p. 136.

[†] Annales de Chimie et de Physique, vol. xxii., p. 494.

[‡] Poggendorff's Annalen, vol. xciv., p. 337.

[§] Philosophical Transactions, 1861, Vol. 151, p. 27, and elsewhere.

[||] Ibid., 1853, Vol. 143, p. 217.

At the beginning of 1859 I definitely attacked this problem, meeting at the outset difficulties and negations the reverse of encouraging. But after some weeks of labour, I found myself in secure possession of the result that gases and vapours exhibited, in relation to radiant heat, phenomena far more surprising than those observed by MELLONI in liquids and solids. On the 26th of May 1859 the subject was brought before the Royal Society,* and on the 10th of June I was able, by illuminating the dial of a galvanometer and casting its image upon a screen, to demonstrate in the Royal Institution not only the fact of absorption, but the astonishing differences of absorption which gases and vapours equally transparent to light manifested in regard to radiant heat.†

The following gases and vapours were then examined:—Air, oxygen, hydrogen, nitrogen, carbonic oxide, carbonic acid, nitrous oxide, coal gas, ammonia, olefiant gas, bisulphide of carbon, chloroform, benzol, iodide of ethyl, cyanide of ethyl, formate of ethyl, acetate of ethyl, propionate of ethyl, iodide of amyl, chloride of amyl, amyline, absolute alcohol, amylic alcohol, methylic alcohol, ethylic ether, ethylamylic ether, sulphuric ether, and some others. In the Philosophical Magazine for 1862 I have given samples of the results obtained with a few of these substances; and I will here confine myself to the remark that were the measurements there recorded multiplied a hundred-fold, they would fall far short of the number actually executed in 1859.

With a view of compelling the feeblest gases and vapours to show, if they possessed it, their capacity to absorb radiant heat, the "method of compensation" was invented.‡ Without prejudice to the delicacy of the galvanometer, this method enabled me to bring into play quantities of heat far greater than those ever previously invoked, my object being so to exalt the total radiation that a minute fraction of that total should reveal itself to experiment. By this method not only were the feebler gases and vapours coerced, but the vastness of the diathermic range, if I may use the phrase, was established with a clearness and an evidence unattainable by any other means then existent.§

Notices of the investigation having appeared in many English and continental journals,* I was induced to defer the detailed publication of the experiments. The investigation itself had taught me the difficulties and dangers which beset it. These had reference both to the methods of experiment and to the purity of the substances employed. To secure the perfect constancy of the sources of heat, and the perfect steadiness of the galvanometer, when the flux of heat was powerful, involved a lengthened discipline. With neither gases nor vapours, moreover, was it easy to obtain uniform results. When generated in different ways, the action of the same gas would sometimes prove itself so discordant as to suggest to me the possible existence of novel allotropic conditions to account for such variations of behaviour. Two samples, moreover, of nominally the same liquid, would furnish vapours yielding results far too divergent to be tolerated. The drying apparatus also contributed its quota of disturbance. These anomalies were finally traced to the fact that an incredibly small amount of impurity derived from the stronger gases or vapours sufficed to disguise and falsify the action of the weaker ones. All this had to be learnt; and when learnt, I thought it desirable, for the sake of accuracy, not to publish the results which had been gained with so much labour, but to go once more, with improved appliances, over the same ground. This I did; though it involved the total abandonment of seven weeks' uninterrupted experimental work in 1859, of seven weeks' similar work in 1860, and of many fragmentary efforts. On the 10th of January, 1861, the memoir containing an account of the investigation was handed in to the Royal Society.†

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* Proceedings of Royal Society, vol. x., p. 37.

† Proceedings of Royal Institution, vol. iii., p. 155.

‡ Philosophical Transactions, 1861, Vol. 151, pp. 6 and 7.

§ With moderate total heats the method of compensation is extremely easy of application; but when the total radiation is very large, some discipline is required to keep the galvanometer needle steady in its most sensitive position. With due training, however, perfect mastery over this difficulty may be obtained.

The first point of importance established in 1859 and developed in the memoir just mentioned was that already referred to; namely, the fact of absorption, and large differences of absorption. The second point—destined, I think, to throw light on the deeper problems of molecular physics—was the proof that while elementary gases offered a scarcely sensible impediment to radiant heat, equally transparent compound gases exhibited, in many cases, an energy of absorption comparable to that of the most athermanous solids and liquids. Determining, for example, the action of a mechanical mixture of two elementary gases, it was proved that without altering either the quantity of matter, or its perfect transparency to light, the absorption of invisible heat might be increased many hundred-fold by the passage of the constituents of the mixture into a state of chemical combination.

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* Proceedings of the Royal Society, May 26, 1859; Proceedings of the Royal Institution, June 10, 1859; Bibliothèque Universelle, July, 1859; Cosmos, vol. 15, p. 321; Nuovo Cimento, vol. 10, p. 196; Comptes Rendus, 1859; and in other journals.

† Section 3 of the Bakerian Lecture for 1861 reveals some of the difficulties which beset the earlier stages of these inquiries. To secure strength of radiation and steadiness of the needle I passed from source to source, obtaining my temperatures in turn from water, oil, fusible metal, sheets of copper heated by regulated flames, and from other things. Approximate results were readily obtainable; but I aimed at a degree of accuracy which would render any material retractation afterwards unnecessary. Soundness of work I thought preferable to rapidity of publication.

A similar deportment may be detected in liquids and solids. The quantity of iodine vapour generated at ordinary temperatures is so small that its action on radiant heat is, as might be expected, insensible. But iodine itself, when liquefied by a powerful solvent, behaves as an almost perfectly transparent body to the obscure calorific rays, even when it is able to extinguish totally the light of the sun. Liquid bromine is also highly diathermanous. The same may be said of phosphorus. In MELLONI'S table, Sicilian sulphur comes next to rocksalt in transmissive power. A concentrated solution of sulphur in bisulphide of carbon exerts no sensible action on radiant heat. By fusing together iodine and sulphur Professor DEWAR has produced a "ray filter" which separates with extreme sharpness the visible from the invisible rays. The remarkable diathermancy of certain specimens of vulcanite, brought to light in the experiments of Mr. GRAHAM BELL and Mr. PREECE, is probably due to the sulphur they contain. MELLONI showed that lampblack is to some extent diathermanous. But when a suitable source of heat is chosen, lampblack proves far more pervious to radiant heat than MELLONI found it to be. An opaque layer of this substance transmits 41 per cent. of the radiation from a hydrogen flame. Were the lampblack optically continuous, the transmission would, doubtless, be still greater. An opaque solution of iodine transmits 99 per cent. of the radiation from the same source, while a layer of pure water 0·07 of an inch in thickness, transmits only 2 per cent. of the radiation from a hydrogen flame. Such results indicate that a profound change in the relation of ponderable matter to the luminiferous ether accompanies the act of chemical combination.

One of my principal aims in the Bakerian Lecture of 1861 was to illustrate the hold which experiment had obtained of a subject previously considered intractable. The densities of the gases and vapours employed were therefore varied within wide limits. In the experimental tube first made use of, a full atmosphere of olefiant gas absorbed more than 80 per cent. of the entire radiation, and it was therefore evident that a small fraction of an atmosphere of such a gas would exert a measurable action. On trial, it was found possible to measure the absorption of $\frac{1}{10,000}$th of an atmosphere of olefiant gas. The action of this gas was determined at sixteen different densities, the absorption, as long as the density was very small, being accurately proportional to the quantity of gas present. Similar experiments were made, and similar results obtained with other gases. The action of sulphuric ether vapour upon radiant heat was proved to be still more powerful than that of olefiant gas. The vapour was first carried into the experimental tube by a current of dry air; and the pure vapour was afterwards examined at seventeen different densities. Bisulphide of carbon was tested at twenty different densities; amylene at ten; benzol at twenty; and so of the others.

Considering the views previously entertained regarding the diathermancy of gases and vapours I was naturally impressed with these results. Sceptical when I first observed them, I scrutinised them closely, until repeated scrutiny abolished every doubt. For my own instruction I illustrated the action of the stronger gases and vapours in a variety of ways. Turning, for example, once rapidly round a cock connecting the exhausted experimental tube with a holder containing a powerful gas, the needle would fly aside, owing to the stoppage of the heat by the infinitesimal amount of gas which entered the tube during the rotation. Discharging a powerful gas or vapour in free air, between the source of heat and the thermopile, a similar energetic action would be produced by the perfectly invisible agent.

I was schooled in such actions before the thought of testing the omnipresent vapour of our atmosphere occurred to me. When it did occur there was in my mind no *à priori* ground for supposing that its action would prove insensible; for why should I assume that $\frac{1}{100}$th of an atmosphere of aqueous vapour would prove neutral, after I had proved a small fraction of this fraction, on the part of other gases and vapours, to be active? There was no reason for such an assumption on my part—nothing to deter me from hopefully submitting the question to experiment. I accordingly tested the water vapour of the atmosphere in which I worked, and found its action on a first trial to be thirteen times that of the air in which it was diffused.

It is not uninstructive to compare this approach to the problem with that of a very distinguished man—the late Professor MAGNUS, of Berlin.* Subsequent to me, he subjected the aqueous vapour of our atmosphere to an experimental test; but he made the experiment under the assured conviction that his result would be negative. "It could," he says, "be foreseen with certainty that the small amount of aqueous vapour taken up by air at ordinary temperatures could exert no influence on the transmission." I think it must be obvious that if MAGNUS had gone through the discipline to which I had been subjected, he would not have used this language. His mistake however was a natural one. In fact during the earlier stages of the inquiry my mind was exactly in the condition of his mind—I also thinking, until practically instructed to the contrary, that the action of aqueous vapour at ordinary temperatures must be immeasurably small. It is well known that MAGNUS tested his foregone conclusion, and found it verified; while I, on the other hand, as above stated, justified mine.

The various gases which had been examined in the experimental tube with regard to their powers of absorption, were next tested as regards their powers of radiation. Columns of the heated gases were allowed to ascend in free air, and to radiate against the pile. In this simple way, the radiative power of "transparent elastic fluids" was for the first time established. The order in which the gases ranged themselves, in regard to radiation, was exactly the order of their absorptions. Here, as in other cases, I instructed myself by observing how gases might be made to play the part of solids. Permitting, for example, a film of one of the stronger gases to glide

* In many respects my generous and helpful friend, but, in regard to this question, my steadfast antagonist for many years.

over a heated surface of polished silver, the radiation from that surface was augmented as it might have been by a coating of isinglass or lampblack.

A surmise has been mentioned regarding new allotropic conditions, as occurring to me amid the perplexities of my earlier experiments. In one instance—that of electrolytic oxygen—the surmise proved correct. My first experiments indicated that the modicum of ozone which went forward with the oxygen, exerted four times the absorption of the gas in which it was diffused.[*] Subsequently, by changing the apparatus, and taking pains to augment the quantity of ozone, the multiple rose successively from 4 to 20, 35, 47, 85, ascending finally to 136.[†] The behaviour of ozone was thus proved to be similar to that of molecules composed of heterogeneous atoms. Hence the conclusion, drawn at the time, that the molecule of ozone was formed of oxygen atoms so grouped as to render their action upon radiant heat virtually that of a compound body. This, it is needless to say, is the constitution now assigned to ozone.

With the view of including corrosive gases and vapours among the number of those examined, and for other reasons, the brass experimental tube was displaced by a tube of glass of the same diameter and nearly 3 feet long. The source of heat was also changed from a LESLIE's cube, containing boiling water, to a plate of copper against which a sheet of flame was permitted to play. Extraordinary precautions were found necessary to insure perfect steadiness on the part of the flame. With this arrangement the practical inability of the elementary gases to absorb radiant heat was further illustrated and confirmed. Chlorine gas and bromine vapour, for example, were proved to be highly diathermanous.

At the pressure of an atmosphere it was found that the diathermic range of colourless gases extended from 1 to about 1000. The portion of gas first entering the experimental tube, having the whole heat to act upon, produced, as might be expected, the greatest effect, the increment of absorption, after a certain quantity of gas or vapour had entered, being infinitesimal.[‡] It was therefore interesting to

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[*] Philosophical Transactions, Vol. 151, p. 8.

[†] Ibid., Vol. 152, pp. 84, 85.

[‡] This is well illustrated by an experiment on sulphuric ether vapour recorded in the Bakerian Lecture for 1861:—

Pressure.	Absorption.
1 inch	214
2 „	282
3 „	315
4 „	330
5 „	330

The absorption of air being taken as unity, that of sulphuric ether vapour at 1 inch mercury pressure is here shown to be 214. When, however, vapour corresponding to a pressure of 4 inches was already in the experimental tube, the addition of another inch did not sensibly augment the absorption.

compare together the various gases at very small pressures. When the pressure was that of 1 inch of mercury the diathermic range was greatly increased, the absorption by olefiant gas being then at least 6000 times the absorption by atmospheric air.

With the changed apparatus the action of the aqueous vapour of our atmosphere was again taken up, and proved to be, not 13 times, as I had at first supposed, but, on fairly humid days, at least 60 times that of the air in which it was diffused. When, moreover, dried air was caused to pass over moistened glass and then carried into the experimental tube, the absorption was still greater.

A power has been claimed for mist or haze which has been denied to aqueous vapour; but in these experiments concentrated luminous beams, which would have infallibly brought into view the least trace of suspended matter, revealed no mist or dimness ot any kind. It is, moreover, demonstrable that an amount of turbidity rendered strikingly evident by a luminous beam, exerts only a fractional part of the action of the pure aqueous vapour. When well dried air was led, not through water or over wet glass, but over bibulous paper, taken apparently dry from the drawers of the laboratory, the amount of vapour carried forward from the pores of the paper produced 72 times the absorption of the air which carried it. After five repetitions of the experiment, wherein the same air was carried over the same paper, a quantity of vapour was still sent forward capable of exerting 47 times the absorption of the air in which it was diffused.

Here the possible action of odours upon radiant heat naturally suggested itself. Many perfumes were accordingly subjected to examination, the odorous substance being in each case carried into the experimental tube by a current of dry air. Thus tested pachouli exerted 30 times, cassia 109 times, while aniseed exerted 372 times the absorption exerted by the air in which it was diffused.

A novel method of exhibiting the absorption and radiation of gaseous bodies, the germ of which had been previously discovered,* was illustrated and developed by the new apparatus. Suppose the experimental tube exhausted and the needle, under the joint action of the two sources, to be at 0°. On admitting a strong vapour the usual deflection would occur. Suppose it to be 50 galvanometric degrees. Let dry air be now introduced until the experimental tube is filled. Although fresh matter is thus thrown athwart the rays of heat, the needle behaves as if the matter within the experimental tube had wholly disappeared. It sinks to zero, and not only so, but passes say to 50° on the other side.

After the first moments of perplexity succeeding the observation of this effect, its cause became clear. On entering the experimental tube the air, having its *vis viva* destroyed, was heated dynamically. Incompetent to radiate itself, it imparted its warmth to the vapour, and this powerful radiator poured the heat thus received against the pile. This heat sufficed not only to neutralise the deflection of 50° due to

* Philosophical Transactions, Vol. 151, p. 32.

absorption, and indicating cold, but to carry the needle up to 50° on the side of heat. So likewise, when the experimental tube was filled with mixed air and vapour, the needle being at 0°, a stroke of the pump, though opening a freer passage for the rays from the source, caused a deflection indicative not of heat, but of cold. Here, the vapour within the tube being chilled by the dilatation of the air, the pile radiated its uncompensated warmth into the vapour and produced the observed deflection.

Such observations suggested a new means of demonstrating the absorption and radiation of heat by gases and vapours. Abandoning all external sources of heat, and permitting the various gases already examined to enter the experimental tube at a common velocity, they became self heated and radiated against the pile. Their radiation, thus determined, corresponded exactly with the results obtained when heated columns of these gases were permitted to rise freely in the atmosphere.

Both the radiation and absorption of vapours were determined in the same manner. The external source of heat was abandoned, and a measured quantity of every vapour was introduced into the experimental tube. Through an orifice of fixed dimensions dry air was then permitted to enter the tube, where the destruction of its *vis viva* raised its temperature. The heated air warmed the vapour, which in its turn radiated the heat imparted to it against the pile. The deflection of the galvanometer declared the strength of this radiation. Absorption was determined by permitting the mixed air and vapour to dilate by a measured quantity, the pile being here the warm body, and the chilled vapour the absorbent. The order in which the vapours stood as regards absorption was here exactly the order of their radiation, while both absorption and radiation, thus determined, agreed with the results obtained by sending the rays from an external source of heat through the pure vapours in the experimental tube.

What has been called "vapour-hesion," whereby liquid films are produced, has been supposed to play a dominant part in my experiments. But it can hardly be imagined that an irregular action of this kind could produce results of such precision and consistency as those here recorded. Such results are, in my opinion, only compatible with the conclusion that the veritable radiators and absorbers are the molecules of the vapours. Apart from all experiment, the notion that vapours must act in this way comes commended to us by the proved, and conceded, deportment of gases. It would be unreasonable to admit that a compound gaseous molecule is active, and at the same time to affirm that a compound vaporous molecule is inert.

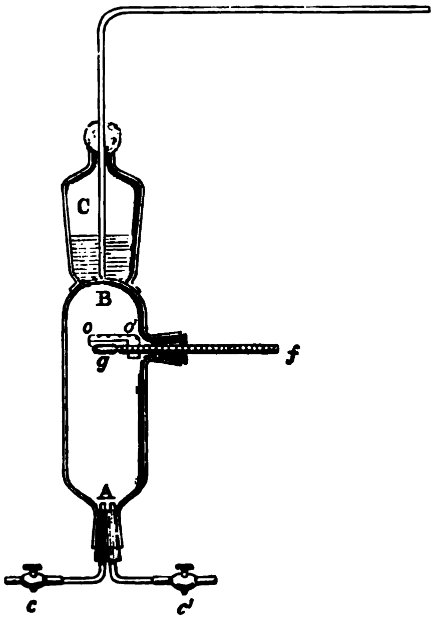
This hypothesis of liquid films formed on the interior surface of the experimental tube, and on the plates of rocksalt, becomes, I think, more embarrassed as we proceed. It depends on the unproved assumption that liquids possess powers of absorption which are denied to their vapours. To water and brine, for instance, MAGNUS largely concedes such powers, but not to aqueous vapour. That the state of aggregation exerts an influence is not denied, but that it is here the dominant factor is open to doubt. To admit this would be to concede that the seat of absorption is the molecule as a whole, to the practical exclusion of the constituent atoms of the molecule. For if the atoms exert any influence, the mere passage from the liquid to the vaporous condition, which separates the molecules from each other, but leaves them individually intact, cannot destroy their powers of absorption.

At an early stage of these researches the parallelism of liquid and vaporous absorption forced itself upon my attention. Thus, my experiments on bisulphide of carbon vapour were connected with the deportment of liquid bisulphide, as set forth in MELLONI'S table. The vapours, moreover, of chloride of sulphur and chloride of phosphorus, whose liquids stand in MELLONI'S table next to bisulphide of carbon, were afterwards proved by me to possess a diathermancy corresponding to that of their liquids. After various references to this subject in preceding memoirs, a portion of the Bakerian Lecture for 1864 was devoted to its examination. Liquid layers enclosed between plates of transparent rocksalt were tested in regard to their diathermancy, and for the sake of control and verification they were employed in five different thicknesses. The vapours of these liquids were examined in quantities proportional to the quantity of liquid, the same quality of heat being applied both to liquids and vapours. By these experiments, it seemed to me placed beyond a doubt, that the liberation of the molecule from the liquid condition did not destroy its absorbent power, the order of absorption being proved to be precisely the same for liquids and their vapours. Ten different substances were shown at the time here referred to to follow this rule. The list has since been extended, and I am not acquainted with a single real exception to the rule. Any reasoning, therefore, which ascribes a potent absorption to perfectly impalpable films, condensed on the surface of my apparatus, and which denies that absorption to the free molecules within the experimental tube, is in my opinion untenable.

The relation between liquids and their vapours here indicated is very thorough. It extends beyond the field of experiment which we have hitherto had in view. I have, for example, published some researches on the action of rays of high refrangibility on gaseous matter, and have shown in a great variety of cases, that the molecules are shaken asunder by such rays. The actinic clouds, as I have called them, produced by this decomposition, reveal vividly the track of the beam by which they are generated, and render it easy to observe the distance to which the action penetrates. In the case of nitrite of amyl, for example, the power of decomposition is soon exhausted, the actinic cloud ceasing abruptly at a point about 18 inches from the place where the beam enters the vapour. An experimental tube 3 feet long, has therefore one half of its vapour shielded by the other half, and on reversing the tube, the shielded half comes instantly down as an actinic cloud. In the case of iodide of allyl vapour, on the other hand, the beam may pass through a charged experimental tube 5 feet long, fill it with an actinic cloud, and still effect decomposition in another tube placed beyond it. What is true of these vapours is true equally of their liquids; for while a layer of the liquid nitrite $\frac{1}{8}$th of an inch thick prevents, when placed in the track of the beam, the decomposition of its vapour, a layer of the liquid iodide, of quadruple thickness, does not arrest the decomposition. The power, and the lack of power, to penetrate considerable depths is shared alike by the liquids and their vapours. Other, and still more subtle and penetrating illustrations of parallelism between liquid and vaporous absorption are mentioned in the Bakerian Lecture for 1864.*

§. 3. *Researches of* Magnus.

Fig. 1.



Prompted by the experiment of Grove, illustrating the chilling action of hydrogen, Magnus, in 1860, began an investigation on the power of gases to *conduct* heat.† His apparatus, traced in outline from his own plate, is shown in fig. 1, where A B is the recipient for the gases, and C a flask containing water kept boiling by a current of steam. The bottom of C, which formed the top of A B, was the source of heat. A thermometer *g f*, shielded by a cork or metal screen *o o* from the radiation of the source, was intended to receive and measure the heat transmitted by conduction. The recipient A B was mounted in a space surrounded by water of a constant temperature. The heating of the thermometer, when A B was exhausted,* was compared with its heating, when A B was filled with various gases, and in every case but one the heating through the gas was found less than the heating through the vacuum. The exception was hydrogen, which carried more heat to the thermometer than was transmitted by the vacuum. The conclusion drawn by its author from this experiment was that hydrogen conducted heat like a metal.

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* Carbonic acid is one of the feeblest of the compound gases, as regards the radiation from solid bodies; but, for the radiation from a carbonic oxide flame, it transcends all other gases in absorbent power. The action of aqueous vapour is also enhanced when it acts upon the rays emitted by a hydrogen flame. The enhancement extends to water. Curious reversals of diathermic position, when heat from different sources is employed, are moreover shown to occur simultaneously with liquids and vapours.

† A Preliminary Note of this inquiry is published in the "Bericht" of the Berlin Academy for July 30th, 1860. No measurements are given, but certain results are announced. The experiments were first published in Poggendorff's Annalen for April, 1861.

One remark only in this Note has any reference to the diathermancy of gases, but it is a significant one. MAGNUS had no doubt as to the power of every one of his gases to conduct heat. There could, he supposed, be only a difference of degree between them and hydrogen. Whence, then, the lowering of the thermometer? He answers thus:—"From this it is not to be inferred that the gases do not conduct heat, but merely that in their case conduction is so feeble as to be neutralised by adiathermancy." These are the only words in the note which have any reference to radiation.

In his next inquiry, MAGNUS dealt directly with the subject of diathermancy, a Preliminary Note of the investigation being published in the Monatsbericht for February 7th, 1861. This note, like its predecessor, consisted of general and descriptive statements, no actual measurements being given. The completed memoir was first published in POGGENDORFF's Annalen for April, 1861. For the purposes of this new inquiry the apparatus used in the experiments on gaseous conduction was modified as shown in fig. 2. To the recipient A B a second one G F was attached, both being connected by the tubulure shown in the figure. The recipient G F rested upon the plate of an air-pump, on which also stood the thermopile *p*, with one of its faces turned towards the source. From the pile, through the air-pump plate, wires passed to the galvanometer. With this apparatus, the absorption by atmospheric air and by oxygen was found to be 11·12, and by hydrogen 14·1 per cent. of the total radiation. The alleged conductivity of hydrogen did not therefore manifest itself in these experiments.

Let us analyse these results. In the first experiments, the distance of the thermometer from the source of heat was 35 millimeters. The action on the thermometer through a vacuum being represented by 100, the action through air and through oxygen of this depth was found to be 82. The loss of 18 per cent. in air and in oxygen was alleged to be due to the adiathermancy of these media, to which percentage, if we wish to ascertain the *total* absorption by air, we should have to add such heat as reached the thermometer by conduction.

Turning now to the modified apparatus, which is evidently drawn to scale, the gas here traversed by the radiant heat was about 275 millimeters in depth, while the stratum traversed in the first experiments was, as stated, only 35 millimeters. Yet in these first experiments an absorption of 18 per cent., while in the later ones an absorption of only 11·21 per cent. is assigned to air. In other words, when the depth of the aerial stratum was augmented more than seven-fold, the absorption, instead of increasing, fell to less than two-thirds of that of the shallower stratum. It is pretty obvious that an influence different from pure absorption came here into play. That influence was convection.

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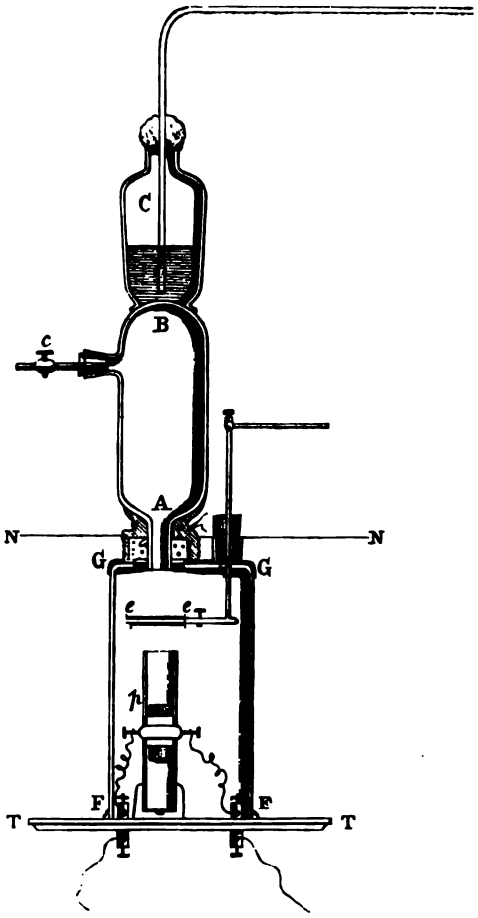
* This vacuum temperature, at least in so far as it exceeded that of the sides of the recipient, was obviously derived from the screen.

Fig. 2.



Anxious to probe this matter to the bottom, and to abolish, or account for, the differences between my friend and myself, I wrote to him proposing an exchange of apparatus—that he should send his to London, and I mine to Berlin. I afterwards had a fac-simile of his apparatus constructed in London, and satisfied myself by actual trial that it was really hampered with the defects I had ascribed to it. By means of the striæ of incense smoke and of chloride of ammonium, the fact of convection in air was rendered plainly visible to the eye, while the behaviour of hydrogen, under like circumstances, revealed the cause of its transporting more heat than the vacuum in the first experiments, and less heat, not only than the vacuum, but than air or oxygen, in the second experiments. In the one case, the thermometer, being close to the source, came within the range of the convection currents of this mobile gas, the heat being transported to it by these currents. In the other case, a considerable distance intervened between the source and the pile, which was further effectually protected by the narrow tubulure. Through it the currents could not pass, but they nevertheless existed in the recipient A B, lowering the temperature of the source without heating the thermoscopic instrument.

The experimental resources of **Magnus** were great, and he here applied them; but the defects of his method were radical and irremovable. These defects reached their culmination in the subsequent researches of Professor **Buff**,* who by pursuing substantially the same method, arrived at the result that a stratum of air 2½ inches thick absorbed 60 per cent. of the radiation from a source of 100° C.† **Buff** also found olefiant gas to be more diathermanous than air, whereas at atmospheric pressures, it is many hundred, and at a pressure of $\frac{1}{30}$th of an atmosphere, many thousand times, more opaque to heat.

This is the point at which aqueous vapour enters into the experiments of **Magnus**. When dry and humid air were compared together in his apparatus, no difference between them was observed. But, apart from all disturbance, it would require an instrumental arrangement far more delicate and powerful than that here employed, to bring into view the action of a stratum of mixed air and aqueous vapour 11 inches deep, and having a temperature of only 15° C. Disturbances, however, were not absent. In the first place, the convection currents which enabled dry air to reduce the radiation by 11·12 per cent., were more than sufficient to mask the action of the vapour. Secondly, dry and humid air were brought in succession into direct contact with the face of the thermopile. The pile was therefore affected by any difference of temperature between it and the air, and it could scarcely be supposed that these temperatures were always alike. It was also affected by the condensation and evaporation which occurred when humid air and dry air were brought successively into contact with its lampblack-coated face. To "vapour-hesion" **Magnus** subsequently ascribed very large effects. Here we have the conditions specially suited to the development of the action, and yet no reference is made to it. Either then the disturbance was overlooked, or the apparatus was not sufficiently delicate to reveal it. To these two sources of disturbance—the lowering of the temperature of the source by convection, and the warming and chilling of the pile by contact, condensation, and evaporation—is to be added another, due to the warming which must have occurred when the dynamically heated air came into direct contact with the thermopile—an action which, in my apparatus, proved sufficient to whirl the needle of the galvanometer more than once through an entire circle.

Magnus next experimented with glass tubes 1 meter long and closed at the ends with plates of glass. His source of heat was a strong gas flame aided, as in the experiments of Dr. **Franz**, by a parabolic mirror. Two tubes were employed, the one blackened within and the other unblackened. With the blackened tube an absorption of 2·44 per cent. was found for air, and an absorption of 3·75 per cent. for hydrogen. In the unblackened tube the absorption by air was 14·75 per cent., and by hydrogen 16·27 per cent. of the total radiation.

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* Phil. Mag., 5th ser., vol. 4, p. 401. For my reply see Proc. Roy. Soc., vol. 30, p. 10.

† Ibid., 1877, vol. 4, p. 424.

I went over this ground with the utmost care, using invisible as well as visible heat. But, substituting plates of pellucid rocksalt for the plates of glass, I failed to realise the effect obtained by MAGNUS. He ascribed the difference between the results obtained with his blackened and his unblackened tube to a change of quality in the heat, produced by reflection from the interior surface of the latter. With plates of rocksalt, however, though the reflection abides, the change of quality does not occur. My position, therefore, in regard to these experiments, is similar to what it was in regard to those of Dr. FRANZ. The results obtained with air, oxygen, and hydrogen, were, I hold, due to the chilling of the heated glass ends of the tube by the cold gases, and the consequent lowering of the secondary radiation.

It was shown by MAGNUS himself, and is moreover obvious at first sight, that the unblackened tube sent a far greater amount of heat to the glass plate adjacent to the thermopile than the blackened one. That plate being more heated by the source, was more chilled by the air when it entered. The greater cooling power of hydrogen accounts, moreover, for the advance of the supposed absorption from 14·75 to 16·27 per cent. With carbonic acid MAGNUS detected a difference which had escaped Dr. FRANZ. Instead of making the action of this gas equal to that of air, he found in the blackened tube an absorption of 8·19 per cent., and in the unblackened tube an absorption of 21·92 per cent. exerted by carbonic acid. Here true absorption mixed itself with the effect of mere chilling, while, with still more powerful gases, the effect of chilling retreated by comparison more and more.

Such were the experiments which determined, in the first instance, the attitude of this distinguished man towards that portion of my work which related to the action of the air and vapour of our atmosphere on radiant heat. In the defence of his position he brought to bear all the resources of consummate skill and large experience. His position, however, was by no means a wholly defensive one. He dwelt repeatedly and emphatically on the dangers—and they are real—to which the method pursued by me was exposed. I had closed my experimental tube with plates of transparent rocksalt and he urged against me the hygroscopic character of this substance. Placing rocksalt beside a vessel of water under a glass shade, he found that it could be rendered dripping wet.* Hence his argument, that, instead of measuring the action of vapour, I had really measured the action of brine. This, however, I could not admit. I was aware of the danger and had avoided it. In many hundred instances the rocksalt plates had been detached from my experimental tube while filled with the very air which had produced the observed absorption, and found to be as dry as polished plate glass. For a week at a time I have charged my experimental tube alternately with dried and undried air, removing every evening the plates of salt while the humid air filled the tube. Their dryness and polish were found unimpaired.* I have frequently flooded the experimental tube with light, and watched narrowly whether any dimness showed itself on the salt, or on the interior surface, when the humid air entered. There was nothing of the kind. I finally abandoned the plates of salt altogether, and obtained in a tube open at both ends substantially the same effects as those obtained when the tube was closed with plates of rocksalt.

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* It has been shown by Professor DEWAR that the exposure of a dry plate of rocksalt for 5 minutes to saturated air sensibly augments the weight of the salt as determined by a delicate balance.

In 1862 MAGNUS came to London. He had been previously working at the points of difference between us, and had strengthened his first conviction. The action of the air he had found to be considerable, and the action of aqueous vapour practically nil. I also had been working, but with an entirely different result. It was hoped by both of us that our differences would be settled during this visit. With my closed experimental tube I showed him the neutrality of dry air and the activity of humid air; and while the latter was in the tube I detached the rocksalt plates and placed them in his hand. He closely inspected them, passed his dry handkerchief over them, and frankly and emphatically pronounced them perfectly dry. I then executed in his presence the experiments with the open tube, and reproduced the results which I had previously published. I subjected the method of compensation to a severe test and showed him how exact it could be made. He frankly confessed his inability to find any flaw in my experiments, and save in one particular made no attempt to reconcile our differences. He accounted for the neutrality of dry air observed by me by pointing to my thermopile, between which and the experimental tube a space of air intervened. He argued, and justly argued, that though the calorific rays were permitted to enter the tube from a vacuum, if the air intervening between tube and pile could produce the effect which he ascribed to it, the heat would be robbed of its absorbable rays before the dry air entered the tube, the subsequent neutrality of dry air being a matter of course. The logic was good, but its basis I knew to be more than doubtful; and I therefore asked him whether a layer $\frac{1}{20}$th of an inch thick between pile and tube would produce any sensible effect. His reply was an emphatic negative. In subsequent experiments, therefore, the conical reflector was removed from my pile, and placed within the experimental tube, its narrow end being caused to abut against the plate of rocksalt. The face of the pile was then brought within less than $\frac{1}{20}$th of an inch of the rocksalt plate; and in this way my former measurements, which had declared the pure air of our atmosphere to be a practical vacuum to radiant heat, were verified to the letter.

The well-earned fame of MAGNUS as an experimenter, and his personal friendliness to myself, rendered it specially incumbent on me to deal respectfully with every one of his suggestions. He once intimated to me that the absorption which I had supposed due to aqueous vapour, might be really due to the smoke and dust suspended in London air. To meet this I carried air myself from the Isle of Wight, had it carried from Epsom Downs and other places, and found the aqueous vapour diffused in such air to be from 60 to 70 times more energetic than the air itself. London air, moreover, was freed from its suspended matter and tested when dry: it proved neutral. The self same air was then rendered humid: its absorbent power was restored. Then, with dry air as a carrier, I introduced smoke into the experimental tube, until it far exceeded in density that suspended in the London air when MAGNUS drew my attention to it. The quantity of heat intercepted by this smoke proved to be only a fraction of that absorbed by the perfectly invisible aqueous vapour.

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* This mastery over the apparatus was not attained without training. Any lapse of care soon declared itself by the condition of the plates of salt.

On his return to Berlin he resumed his labours. He had been especially impressed by the experiments with the open tube, and to this point he directed his chief attention. "The result of this experiment," he writes, "was so surprising and so little in accord with what I had found by other methods, that on reaching home I determined to repeat the experiment." He did so, with this result:—"I have," he says, "repeated the blowing in of dry air and moist air many hundred times; but in no single case was the deflection such as to indicate a greater absorption by moist air." * Humid air in his experiments produced the deflection of heat; dry air the deflection of cold—a result diametrically opposed to mine. In London he had seen that my deflections were as large as I had affirmed them to be, but he had not criticised them with a view of ascertaining whether they were or were not in the right direction. In these new experiments, however, he had, he thought, hit upon their origin. The moving air had reached the face of the thermopile, producing, when humid, heat by condensation, and when dry, cold by evaporation.

I read the account of these experiments with some concern; for it was thereby made plain to me that MAGNUS had by no means realised the anxious care that I had bestowed upon my work. The testimony of an independent observer would, I thought, set the matter right. My apparatus, carefully adjusted, was accordingly handed over to Dr. FRANKLAND, who minutely tested every point involved in, or arising out of, the objection of MAGNUS. He verified all my results. His opinion as to the accuracy of the method of compensation is worth recording. "In conclusion," he writes, "I cannot but express my surprise and admiration at the precision and sharpness of the indications of your apparatus. Without having actually worked with it I should not have thought it possible to obtain these qualities in so high a degree in determinations of such extreme delicacy."† To this may be added the subsequent testimony of Professor

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* POGGENDORFF'S Annalen, 1863, vol. 118, p. 580; Phil. Mag., 1863, vol. 26, p. 25.

† The total heat here employed amounted to 86·2° of a quadrant. This exceedingly large deflection was neutralised by the radiation from the compensating cube. But so accurately were the two sources balanced, and so constant was the radiation on both sides, that the determinations were made with ease, and without sensible disturbance or fluctuation.

WILD, now of the University of St. Petersburg, who went carefully over the same ground. "In all my experiments," he says, "conducted according to TYNDALL's method, which included more than a hundred distinct observations, I have never obtained deflections of the galvanometer needle in contradiction to the statements of Professor TYNDALL."*

In an extremely able paper, a translation of which is published in the Philosophical Magazine for October, 1866, the Petersburg philosopher compares the methods pursued by MAGNUS and myself respectively. Insufficient sensitiveness and the disturbance due to convection currents, caused him, he says, to abandon the method of MAGNUS. "Although," he continues, "this method of investigating absorption may, in the hands of so experienced and expert an experimenter as Professor MAGNUS, be an appropriate one for determining absolute values with great certainty, I feel bound, from my own experience, to give a decided preference to TYNDALL's method, not only on account of the greater facility with which it furnishes qualitative [quantitative] results, but also in consequence of its greater delicacy. It is principally in consequence of this greater delicacy that, notwithstanding the negative results furnished by MAGNUS's method, I maintain that the greater absorptive power of moist air, as compared with dry, has been fully established by the experiments made according to TYNDALL's method; and I am of opinion that meteorologists may without hesitation accept this new fact in their endeavours to explain phenomena which hitherto have remained more or less enigmatical."

In 1866 MAGNUS varied his method of experiment, seeking to solve the question of absorption by observations on radiation. "I have," he says, "made a few determinations of the radiation of dry and moist air, and of some other gases and vapours. Up to the present time," he continues, "the capacity of these bodies to transmit heat has alone been determined."† He then describes his arrangement:—"The gases and vapours were passed through a brass tube of 15 millimeters internal diameter, which was placed horizontally and heated by gas flames. One end of the tube was bent upwards so that the heated air ascended vertically, while at a distance of 400 millimeters from the vertical current, was placed the thermopile." When dry air was sent through this tube, the deflection produced was three divisions of a scale; when air which had passed through water at a temperature of 15° C. was sent through the tube, the deflection rose to five divisions; when the water was warmed to 60° or 80° FAHR., the deflection was 20 divisions; and when the water boiled, the deflection was 100 divisions. In this last experiment, however, a mist appeared, so that, as urged at the time, the radiation could not be said to have been purely from vapour. In the other cases no mist was visible, but it was nevertheless concluded that the 20-division deflection was due to the formation of mist at the boundary of the ascending current.

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* Phil. Mag., 4th ser., vol. 32, p. 252.

† This is an inadvertence. Exhaustive experiments on the radiation of gases and vapours had been made and published many years previously.

I should be disposed to claim these experiments as telling in my favour. The first of them, in my opinion, dealt with the radiation not from dry air but from the adjacent aqueous vapour which had been warmed by the dry air. That the deflection in the second experiment was small is not surprising. The radiation which could reach the pile from a jet of air only 15 millimeters in diameter, and containing such moisture as could be taken up at 15° C., must have been extremely small under any circumstances. But in the present case, even this small radiation was diminished by the passage of the heat through 400 millimeters of undried air. I should demur to the explanation of the third experiment, and question the warrant to imagine a mist which could not be seen. Even the fourth experiment, where mist was visible, yielded, I doubt not, a mixed result; part of the effect, and probably the smallest part, being due to the mist, and part of it to the vapour.

With regard to the radiation from hot aqueous vapour, the following experiment is typical of some hundreds which I have had occasion to make. A burner consisting of two rings provided with numerous small apertures was placed within a square tin chimney. At some height above the burner the chimney was perforated so as to enable the radiation from a heated gaseous column within the chimney to reach a distant thermopile. The side of the chimney facing the pile was so protected by screens that the radiation from the chimney itself was nil. Connecting the burner with a bottle of compressed hydrogen the gas was ignited. A column of hot vapour rose from the burner and passed the aperture in the chimney, through which it sent its rays to the pile. Mere tips of flame were first employed; the column of vapour rising from them sufficed nevertheless to produce a permanent deflection of

40°

A slight augmentation of the flame sent the needle up to

60°

A still further augmentation sent it up to

75°

This last deflection was equivalent to more than 400 of the degrees in the neighbourhood of zero.

The radiating column was here considerably above the flame. To examine the condition of the column a concentrated luminous beam was directed upon it. There was no precipitation. On the contrary, the suspended matter in the air of the chimney was much less than that of the surrounding air. Instead of a white mist, we had the blackness due to the destruction of the floating matter by the hydrogen flame.

On quenching the flame the needle returned accurately to zero.

In his objections MAGNUS, for the most part, dealt with true causes, but he erred as to their scope of action. I never denied the existence of the dangers which he emphasized. The hygroscopic character of rocksalt, for example, to which he recurred so often, cannot be questioned. It has a strong attraction for moisture, especially when cold. On this point my experience has been large, and I applied it in the execution of my experiments. These, as I have so frequently stated, were conducted with plates of salt as dry as polished glass or rock crystal. Thus, while conceding the fact that rocksalt is hygroscopic, I demur to its application.

A similar remark applies to the last solution offered by MAGNUS of the differences between us. In 1867, he showed that vapours were condensed by surface attraction to a greater extent than had previously been supposed. Blowing vapour-laden air into a metal tube, he found that heat was generated. He inferred, and rightly inferred, that this heat was produced by the condensation which occurred on the interior surface. This condensation he found to depend on the condition of the surface, being greater when it was tarnished or coated than when it was polished. He saturated air with moisture at a temperature of 16° C., and then raised both it and his pile to a temperature of 38°. When such air was blown against the dry face of the pile heat was generated. Condensation therefore occurred upon a surface 22° C. higher than the dew point of the vapour. Against this I have nothing to urge. But the fact by no means justifies the inference drawn from it, which was, that the vapours in my experimental tube were converted by "vapour-hesion" into liquid layers of high opacity to radiant heat. These layers, acting upon the calorific rays impinging on the interior surface of my tube, produced the absorption which I had erroneously ascribed to the vapours. More than this, the liquid layers were supposed to be broken up into discontinuous patches which not only absorbed the heat but scattered it. "Vapour-hesion," it may be added, was found to vary with the liquid which produced the vapour, being particularly strong in the case of alcohol.

MAGNUS brought this generalization to the test of experiment, but failed to verify it. He urged humid air against a dry mirror, from which radiant heat was reflected; but unless he wetted the mirror visibly no effect was produced on the reflected beam. Still he held that reflection, oft repeated, rendered sensible an action which eluded a single reflection. My position here is clear. I do not doubt surface attraction, or deny the existence of impalpable films. No experiment was ever made on the reflection of light or radiant heat in which such films did not intervene, but they had as little effect upon my results as they had upon those of DE LA PROVOSTAYE and DESAINS,* and of other refined experimenters. As early as 1859, I was made aware of the danger which might arise from condensation. Warned by the action of chlorine on my brass experimental tube, I coated it inside with lampblack, and re-tested with it all my vapours. The result removed from my mind the suspicion that surface condensation had anything to do with the observed absorption. Many similar experiments with blackened tubes were subsequently made by me for my own safety and instruction. There was no substantial difference between the results obtained with such tubes, and those obtained with polished tubes in which internal reflection came into play.

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* Considering the energy of water as a radiator, exceeding according to LESLIE that of lampblack itself, the film of this liquid which must have covered the plates of silver in the experiments of the two French philosophers ought, if MAGNUS be correct, to have sensibly raised the emission. Calling the emission from lampblack 100, that from polished silver plus the film was only 2·1.

Such are the general features and phases of a discussion which, though dealing only with a small item of my work, has consumed a considerable amount of time. Other able experimenters have entered this field, the latest of whom, MM. Lecher and Pernter, have published a long and learned memoir in Wiedemann's Annalen, which has been translated in the Philosophical Magazine for January, 1881. My experiments with gases they corroborate, but not those with vapours. Regarding the action of aqueous vapour they are especially emphatic, their conclusion being: "that moist air does not perceptibly absorb the heat rays from a source of 100° C." In fact they found moist air a little more transparent than a vacuum. "No imaginable source of error," they affirm, "has here been left out of account." The arrangement for filling with moist air was varied, the air allowed to stand for a long time over the water in the gas-holder, and this moist air then passed through several wash-bottles into the experimental space, but with the same *negative* result." In common with Magnus, MM. Lecher and Pernter ascribe my results to the condensation of liquid films on the rocksalt plates, and on the polished inner surface of my tube.*

§. 4. *Experiments resumed : Verifications.*

With a view to my own instruction, and to the removal of uncertainty from other minds, these researches on radiant heat were resumed in November, 1880. A brass experimental tube 4 feet long, $2\frac{3}{8}$th inches in diameter, and polished within, was first employed.† Interposed between it and the source was a "front chamber" through which, when exhausted, the rays passed into the experimental tube. A plate of transparent rocksalt separated the tube from the chamber; while a second plate of salt closed the distant end of the experimental tube. The source of heat was at first a Leslie's cube containing water at 100° C., to one of the faces of which the end of the front chamber was carefully soldered. The chamber also passed air-tight through a copper cell in which a continuous circulation of cold water was kept up. The heat which might otherwise have reached the experimental tube by conduction from the source was thus cut off. One face of a thermopile, provided with a reflecting cone, received the rays which passed through the experimental tube. The other face, also provided with a cone, received the rays from a "compensating cube," used, as formerly, to neutralise the radiation from the source, and to bring the needle of the galvanometer to zero when the experimental tube was exhausted. On the entrance then of any absorbent gas or vapour the equilibrium was destroyed, the needle moved from zero, and from the observed permanent deflection the absorption was calculated. Other qualities of heat, and other experimental tubes than that here described, were afterwards introduced into the inquiry.

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* However I may otherwise differ from MM. Lecher and Pernter, I agree with their opening remark, that few other questions of experimental physics present difficulties so great as the one here under consideration. Nor do I see reason to differ from their closing words, that "the extraordinary difficulty of investigations of this sort would be richly repaid by the attainment of quantitative results; whilst the corresponding optical investigations (immeasurably easier) will always remain more of a qualitative nature." It is the difficulty here signalised that has caused so many distinguished investigators to go astray in this field of inquiry. I may state here that on the receipt of their paper I wrote to MM. Lecher and Pernter, but my communication was returned from Vienna through the dead letter office.

† The plate answering to this description will be found in the Philosophical Transactions for 1861.

I here give the measurements executed in 1880 with the vapours of nine different liquids, in experimental tubes of the dimensions above given.

TABLE I.

		Vapours.		Liquids.
	Pressures.	LESLIE's cube vacuum.	LESLIE's cube free.	Red hot spiral.
Bisulphide of Carbon	·48 in. mer.	4·4	5·0	7·6
Chloroform	·36 „	12·8	12·9	28·8
Benzol	·32 „	14·8	15·0	44·5
Iodide of Ethyl	·36 „	18·4	19·3	47·0
Iodide of Methyl	·46 „	25·0	26·2	59·0
Amylene	·26 „	26·1	27·2	65·0
Sulphuric Ether	·28 „	35·0	35·6	71·0
Acetic Ether	·29 „	43·3	43·7	77·5
Formic Ether	·36 „	43·3	44·0	78·0

The "pressures" in this table are chosen with a view to the comparison of liquids and vapours. They express quantities of vapour which are proportional to the quantities of matter in the respective liquids at a common thickness. The two next columns contain the absorptions per 100, of the heat from two LESLIE's cubes, the one with a vacuum in front of it, the other placed in free air, and well protected from air-currents. The close agreement of the two columns proves the "front chamber" to be a superfluity. It also illustrates the coincidence to be attained in these measurements when they are carefully made. In the last column I have placed the absorptions exerted by liquid layers of the respective substances at a common thickness of one millimeter. The source of heat here was an incandescent platinum spiral. The order of absorption of liquids and vapours is the same.

This order is, as might be expected, undisturbed, when we apply heat of the same quality to liquids and vapours respectively. This is shown by the following table:—

TABLE II.

	Polished tube.		Blackened tube.
	Spiral (dull).	Spiral (bright).	Spiral (dull).
Bisulphide of Carbon	3·0	2·4	3·0
Chloroform	7·0	4·6	5·0
Benzol	9·4	7·5	10·0
Iodide of Ethyl	13·6	8·6	12·3
Iodide of Methyl	15·6	10·7	13·8
Amylene	20·6	14·3	17·5
Sulphuric Ether	27·0	21·0	23·8
Acetic Ether	33·0	23·0	29·0
Formic Ether	33·0	23·0	30·0

The vapour pressures here are the same as those employed in Table I. The order of absorption is the same in both tables; but its amount is diminished. This was to be expected from the difference in quality of the heat. We are dealing with transparent vapours—in other words, vapours pervious to the luminous rays; and the greater the proportion of these rays in the calorific beam, the less will be the absorption. This is well illustrated by the second column in the table, which shows the fall of the absorption when the spiral is raised from a dull red to almost a white heat. The polished tube was used in both these cases.

The third column of figures in Table II. shows the results obtained when the experimental tube was coated within with lampblack. The absorptions are in the same order, and almost of the same amount as those of the first column. The case is representative and might be multiplied to any extent. It is incompatible with the notion that my results were due to films collected on the polished interior surface of my experimental tube.

Placing the substances recently experimented on in the order of their absorption, and also in the order which they exhibited in 1864, we have the following two columns:—

1880.	1864.
Bisulphide of Carbon	Bisulphide of Carbon
Chloroform	Chloroform
Benzol	Iodide of Methyl
Iodide of Ethyl	Iodide of Ethyl
Iodide of Methyl	Benzol
Amylene	Amylene
Sulphuric Ether	Sulphuric Ether
Acetic Ether	Acetic Ether
Formic Ether	Formic Ether

In 1864, therefore, the iodide of methyl proved itself more diathermanous than the iodide of ethyl, while both of them were more diathermanous than benzol. In 1880, the case was precisely the reverse. Suspecting that the discrepancy might be due to impurity, I requested my friend Professor DEWAR to subject the liquids to a further process of purification. Tested afterwards, they produced the following deflections :—

	A	B	C
Bisulphide of Carbon	5·0°	5·0°	4·0°
Chloroform	17·0	15·0	6·0
Iodide of Methyl	39·4	33·0	8·0
Iodide of Ethyl	33·0	35·0	12·5
Amylene	42·0	41·0	16·0
Sulphuric Ether	44·3	43·5	18·2
Acetic Ether	46·2	45·5	22·0
Formic Ether	47·5	46·9	22·2

Under A and B respectively are the deflections produced by the liquids prior to and after purification. The iodide of methyl falls from 39·4° to 33°, while the iodide of ethyl rises from 33° to 35°. The relative positions occupied by the liquids in 1864 are thus restored. Benzol, however, remained permanently lower than before. Under C are the deflections produced by the vapours of the purified liquids. Here also the positions of the two iodides are reversed, vaporous absorption following the order of liquid absorption. I have frequently encountered cases of this character. The simultaneous change of diathermic position of liquid and vapour indicates that the foreign ingredient, whatever it was, possessed approximately the same volatility as the substance which it vitiated.

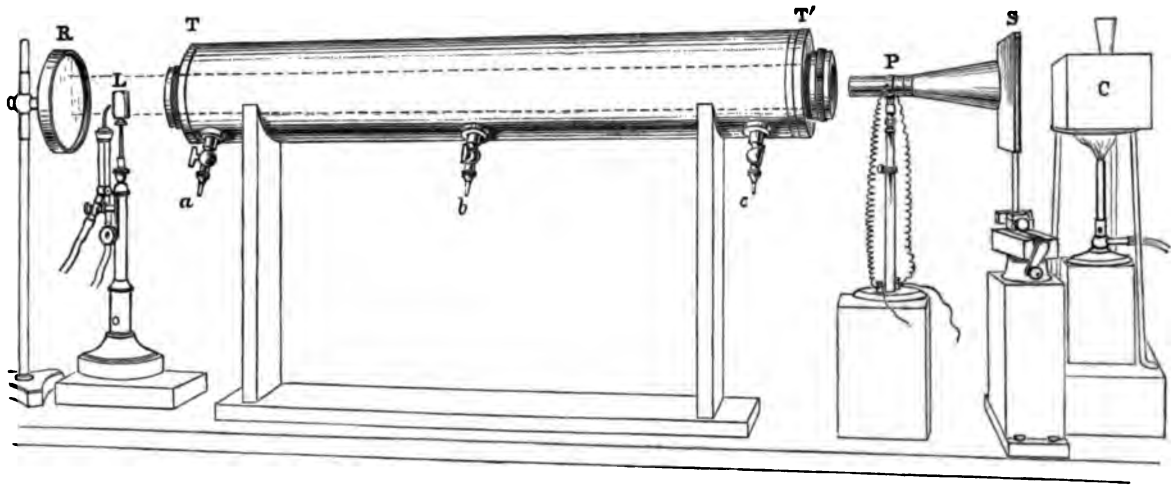
§. 5. *New Experiments. Hypothesis of Internal Films.*

a. The Experimental Tube.

I wish now to come to closer quarters with the argument urged by MAGNUS and repeated by MM. LECHER and PERNTER, namely that my results were due to "vapour-hesion"—that is to say to liquid films condensed on my experimental tube and on my plates of rocksalt. The two investigators last named express unqualified surprise that I should have neglected the simple precaution of experimenting with blackened tubes. But this precaution was by no means neglected by me. I have repeatedly fortified myself by experiments of this character, with the result recorded in Table I. But I went further. A smooth coating of lampblack, however powerful as an absorber, might be held competent to reflect a certain portion of the incident heat. Hence my desire to get entirely rid of reflection, by avoiding all contact with the interior surface of the experimental tube.

In fig. 3 an apparatus is sketched by means of which this has been accomplished. T T′ is a stout tube of brass, 36 inches long and of 6 inches internal diameter. Projecting from its otherwise closed ends are screws 1 inch each in depth, surrounding circular apertures $3\frac{1}{8}$ inches in diameter. By means of screw caps these apertures may be closed air-tight by transparent plates of rocksalt, the plates being protected from crushing by washers of indiarubber. *a* is a cock leading to an air-pump and a barometer tube, *b* is a cock to which flasks can be attached, while *c* is a cock connected with a purifying apparatus (not shown). This consists of a U tube filled with fragments of clean glass moistened with sulphuric acid, and of a second tube filled with fragments of Carrara marble wetted with caustic potash. A plug of cotton wool intercepts the floating matter of the air. The source of heat L is a cylinder of carefully prepared lime, against which a flame of coal gas and oxygen impinges. The incandescent lime faces the concave mirror R, which receives the rays and sends them back in a parallel beam through the tube T T′. P is the thermopile from which wires proceed to a distant galvanometer, the reflected dial of which is observed through a telescope. S is an adjusting screen. At C is what, in my former researches, I have called the "compensating cube," used to neutralise the radiation from the source L, and to bring the galvanometer needle to zero. The chief object of this arrangement is to enable the experimenter to send a calorific beam along the axis of the tube T T′, which shall never touch its internal surface. The great width of the tube, aided when necessary by diaphragms, renders this easy of accomplishment.

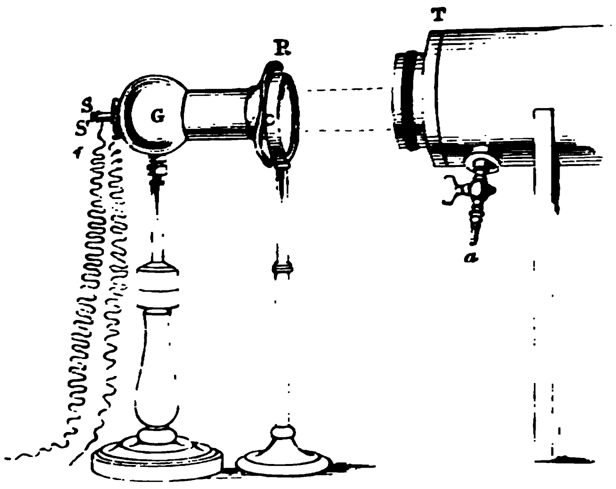
Fig. 3.



The mode of operation is readily understood. Suppose the heat impinging on the pile to produce a galvanometric deflection of 50°. This, the "total heat," is neutralised by the radiation from the compensating cube. To render the compensation accurate, the double screen S is shifted by an extremely fine screw motion. Even when the total heat is very large it is possible, by means of this screen, to neutralise accurately the radiation of the source and bring the needle to zero. Suppose it to be there when the tube T T′ is exhausted; on permitting a gas or vapour capable of absorbing radiant heat to enter T T′, the pre-existing equilibrium is destroyed; the needle moves promptly away from zero, and from the observed deflection the absorption may be calculated.

Instead of the concave reflector, a rocksalt lens of great purity is sometimes used to render the beam parallel. When the lens is used, the incandescent portion of the cylinder of lime is caused to face the tube T T′, between which and the lime the lens is introduced. Instead of the incandescent lime, a spiral of platinum wire heated to redness by a voltaic current is frequently employed as the source of heat. The spiral is placed in the centre of a brass globe G, fig. 4, and is connected with the screws S S′ fixed in the back of the globe, from which wires pass to the battery. R shows the position of the rocksalt lens, while one end of the experimental tube is shown at T, The greatest care is here necessary to protect the spiral from any agitation of the air. Such care, however, it is always in the experimenter's power to bestow. To secure constancy in the radiation, the battery was charged three times a day, the strength of the current being regulated by a rheostat, and checked by a tangent galvanometer.

Fig. 4.



An experimental tube of the description shown in fig. 3 was first employed by me about twelve years ago, and with it I then verified all the experiments I had previously made on the absorption of radiant heat by gases and vapours. The experiments were not published in any scientific journal, but they are thus referred to at page 394 of my volume of collected memoirs, entitled "Contributions to Molecular Physics in the domain of Radiant Heat":—"The two ends of an experimental tube 38 inches long and 6 inches in diameter were each provided with an aperture 2·6 inches in diameter. These apertures were closed with plates of rocksalt. The source of heat was a platinum spiral, well defended from air-currents, and heated to redness by an electric current. In front of the spiral was a rocksalt lens, which sent a slightly convergent beam through the tube. Behind the most distant plate was formed a sharply defined image of the spiral, its size being such *that it was wholly embraced by the plate of salt.* Here then was a beam of heat passing through an experimental tube without coming into contact either with the surface of the tube itself, or with any coating or lining of that surface. With this apparatus all my old experiments on vapours have been frequently repeated. There is no substantial difference between the results thus obtained, and those obtained with an experimental tube, where nineteen-twentieths of the heat which reached the pile, was reflected heat."

The tube referred to in this extract was of rough brass, tarnished within. Hence, when air entered it, after exhaustion, the dynamic heating of the tube and the partial condensation of the vapour when the air was moist, produced an amount of radiation from its internal surface which, though small, was a source of some disturbance. In my present experiments, therefore, another tube of the dimensions above given is employed, and to reduce to a minimum any radiation from its internal surface, it is coated within with silver, deposited electrolytically and highly polished. Experiments with this tube show that it does not in any way disturb the true radiation from the source.

In Table I. are recorded five series of measurements executed with a brass tube polished within. But as the liquids, though reported pure by the manufacturing chemist, vary slightly from time to time, I thought it advisable to adhere to the same samples in experiments wherein a tube with reflecting interior is to be compared with one permitting of no reflection. The following five series of new measurements were therefore executed with the tube first mentioned.

Table III.

Brass tube with internal reflection.

	Pressures.	A Spiral (dark).	B Spiral (bright).	C Lime (free).	D Lime (mirror).	E Lime (lens).
Bisulphide of Carbon . .	·48	5·0	3·1	3·4	3·1	2·5
Chloroform.	·36	7·9	5·3	5·6	4·8	5·0
Benzol	·32	9·6	8·6	7·9	6·9	6·3
Iodide of Methyl . . .	·46	12·1	8·8	10·2	8·1	7·5
Iodide of Ethyl	·36	15·0	11·9	12·5	11·3	10·6
Amylene	·26	21·9	15·6	16·0	14·4	12·5
Sulphuric Ether	·28	30·9	23·1	22·7	19·8	18·1
Acetic Ether	·29	36·9	27·3	29·6	24·1	. .
Formic Ether.	·36	. .	. .	29·6	25·0	23·8

Column A in this table contains the absorptions of the respective vapours, in hundredths of the total radiation, when the source was a platinum spiral just under incandescence, unaided by either lens or mirror. Under B are the absorptions when the source was the same spiral heated to bright redness. The same vibrating atoms are preserved, but in A they vibrate on the whole more slowly than in B; and as the atomic periods of the transparent vapours synchronise best with the slower oscillations, the absorptions in A are considerably higher than those in B. Under C are the absorptions when the source was a moderate limelight, produced by coal gas and oxygen. Under D are the absorptions when the source was the limelight, with its heat gathered up and sent through the experimental tube by a concave mirror, while under E are the absorptions of the heat of the incandescent spiral aided by a rocksalt lens. The absorptions in D and E are somewhat less than those in C, because the path of the rays was diminished, through the reduction of internal reflection by the mirror and lens.

With the foregoing results, obtained with the brass experimental tube polished within, where the greater portion of the heat reaching the pile had undergone reflection, are now to be compared those obtained with the silvered tube, where internal reflection was wholly avoided.

TABLE IV.

WIDE silvered tube with no reflection.

	L	L	M	M	L
	A	B	C	D	E
	Spiral (dark).	Spiral (bright).	Lime (bright).	Lime (moderate).	Lime (dull red).
Bisulphide of Carbon	4·0	1·8	1·5	1·5	3·3
Chloroform	5·6	3·0	3·5	4·3	6·0
Benzol	7·0	5·1	4·4	6·5	8·0
Iodide of Methyl	7·5	5·9	5·0	6·5	9·2
Iodide of Ethyl	11·3	7·6	6·8	8·5	11·2
Amylene	15·0	11·8	8·8	12·0	17·3
Sulphuric Ether.	21·2	17·1	12·5	15·0	25·0
Acetic Ether.	26·0	20·0	16·3	24·0	32·0
Formic Ether	27·0	21·0	17·0	25·0	34·0

The vapour pressures here were those given in Table III. Column A contains the absorptions measured when the source of heat was a spiral under incandescence, aided by a rocksalt lens. B contains the absorptions measured with the same arrangement, the spiral being raised to bright redness. As usual the heat of lowest refrangibility is most absorbed. C contains the absorptions of a tolerably intense limelight aided by the silvered mirror. D the absorptions of the same light with its intensity reduced, while E contains the absorptions of the rays from a lime cylinder heated to dull redness by a steady flame of hydrogen, burning in air, and aided by a rocksalt lens.

Here, then, where no trace of reflected heat is sent to the pile, we have substantially the same results as those obtained when by far the greater portion of the heat reaching the pile had undergone reflection. The agreement shows that the films, the action of which had been posited, but never proved, are, in regard to this action, what I always knew them to be, imaginary.

b. The Plates of Rocksalt.

Having thus, I trust effectually, disposed of the hypothesis which ascribed my results to liquid layers covering the interior surface of the experimental tube, I proceed to the examination of the plates of rocksalt which have also been credited with a liquid deposit. This hypothesis is to a great extent disposed of in a paper of mine published in the Philosophical Transactions for 1864; which, like other papers, has been overlooked by writers on this question. An apparatus is there described which enabled me to operate upon gaseous strata of different thicknesses, the thickness, in each case, being measured with great accuracy by aid of a vernier. Let two polished plates of rocksalt be supposed in close contact with each other, and let them be gradually separated by a suitable rack-and-pinion movement. From the first moment of their separation let the space between them be copiously supplied with vapour. Liquid films, if they form at all, will be deposited when the distance between the plates of salt is but small. As far as the films are concerned, they will be as influential when the plates are $\frac{1}{20}$th of an inch apart as when they are 2 inches apart. Hence, if the hypothesis of my opponents be correct, the absorption ought to declare itself with the former amount of separation as clearly as with the latter. But if, as I allege, the absorption be the act of the vapour molecules, then the deepening of the vapour stratum will be accompanied by an increase of the absorption. The following experiment bearing directly on this subject was executed in 1864. My plates of salt were first fixed at a distance of $\frac{1}{20}$th of an inch asunder, and the space between them was copiously supplied with air saturated with sulphuric ether vapour. The distance between the plates of salt was then augmented by steps, as shown in the following table, and at each step the absorption was determined. Here are the results:—

Thickness of vapour stratum.	Absorption.
0·05 inch	2·1 per cent.
0·1 ,,	4·6 ,,
0·2 ,,	8·7 ,,
0·4 ,,	14·3 ,,
0·8 ,,	21·0 ,,
1·5 ,,	34·6 ,,
2·0 ,,	35·1 ,,

We thus see that when the depth of the vapour stratum augments from $\frac{1}{20}$th of an inch to 2 inches, the absorption augments from 2 per cent. to 35 per cent. of the total radiation.

It is only with sulphuric ether that an absorption of 2 per cent. by the thinnest stratum could be obtained. With most other vapours the action of a layer $\frac{1}{20}$th of an inch in thickness proved insensible. We may thus begin with a layer which yields an absorption immeasurably small, gradually augment the thickness of the absorbing layer until it becomes a column 38 inches long, and find, throughout, the increase of absorption running hand in hand with the increase of the length of the absorbent layer. This result is utterly irreconcilable with the hypothesis that liquid layers on the plates of salt played any important part in my experiments.

With regard to "vapour-hesion," I have to make the following further remarks. Let a thin plate of polished rocksalt be placed upon, or against, the face of the thermopile, with its lampblack removed, so as to expose a clean metallic surface. Let an open glass tube end about a quarter of an inch above the plate of salt; and through this tube let a current of mixed air and vapour be gently urged downwards against the plate. No fine spray, which might readily arise from bubbling through a liquid, is to be permitted to mingle with the vapour. My vapours, I may say, have been usually formed, without bubbling, in large flasks, each containing a portion of a volatile liquid, the vapour of which was permitted to diffuse in the air of the flask. FLETCHER'S foot-bellows were employed to urge the vapours forward. The needle being at zero, or thereabouts, on causing the mixed current to impinge upon the rocksalt, the needle immediately swings aside, the deflection varying from 20° to 80° and more, according to the delicacy of the galvanometer and the quality of the vapour.

From such experiments, which prove heat to be liberated when vapour comes into contact with the salt, the condensation of the vapour has been inferred. I accept the inference.

This, however, does not imply the acceptance of the inference from this inference, that the condensed films exert the action ascribed to them on radiant heat. To test whether they do so or not I had a circular plate of rocksalt mounted on its edge, and so placed that a beam of heat passed through it normally to a distant thermopile. The source of heat was an incandescent spiral, aided by a concave mirror; and the total radiation, after passing through the salt and impinging on the pile, produced a deflection of 51°. This radiation was accurately neutralised by a compensating cube, the needle under the operation of the two opposing forces pointing to 0°. By an arrangement which every experimenter can imagine for himself, sheets of air, laden with various vapours, could be poured in succession over the plate of salt. If, under these circumstances, absorbent films were formed, the equilibrium would be destroyed and the needle would move from 0°. My slit, which was the flattened base of an open tin cone, was placed across the upper part of the plate of rocksalt, so that the vapours, which were all heavier than air, should fall over the plate downwards. Here are the results obtained when the following vapours were permitted to form films upon the plate:—

Bisulphide of Carbon	A barely sensible action.
Chloroform	No action.
Benzol	No action.
Iodide of Methyl	No action.
Iodide of Ethyl	No action.
Amylene	No action.
Sulphuric Ether	A barely sensible action.
Acetic Ether	No action.
Formic Ether	No action.
Alcohol	No action.

The minute deflections produced by bisulphide of carbon and sulphuric ether had nothing to do with liquid films, so that the words "no action" might have been written against these substances as against the others. While therefore recognising the fact of condensation, these simple experiments prove how incorrect it is to credit the condensed films with the effects which have been ascribed to them. According to MAGNUS, alcohol exhibits a force of vapour-hesion particularly strong: but we here see that even this vapour produces no sensible effect. The vapours have been tested with other sources of heat, with the same result.

The deportment of dry and humid air is, as usual, very instructive. When the thin plate of rocksalt resting on the naked face of the pile, was exposed to the common air of the laboratory, it, of course, contracted a film corresponding to the humidity of the air. The sweeping away of this film by dry air produced a very small deflection indicating cold. When, on the contrary, humid air was urged against the salt, the deflection indicating heat was prompt and large. We have now to test the action of the liquid film thus formed upon radiant heat. The circular plate of rocksalt before referred to was mounted, with the incandescent spiral on one side and the pile on the other. The slit was so arranged that dry and humid air could be sent in succession over the surface of the plate. As before, the radiation from the spiral was neutralised by the compensating cube, the needle pointing to zero when the two sources were in equilibrium. On sweeping dry air over the plate of salt, so as to remove the film contracted from the laboratory air, there was no motion of the needle. On pouring humid air from the slit over the salt, there was no motion of the needle. To moisten the air it was urged from bottom to top through a tall jar filled with wet bibulous paper. The same apparatus had furnished humid air which produced a swing of 80° when urged against the thin plate of rocksalt resting on the face of the pile. As a barrier to radiant heat it was nevertheless powerless. The condensation may even be considerably enhanced without producing any sensible effect. Through a glass tube I urged my breath against the plate of salt so as to produce the colours of thin plates, without any sensible effect upon the galvanometer.

Having thus clearly shown what films, even in an exaggerated form, cannot accomplish, I hope, in testing the action of humid air, that the use of the silvered tube, closed with polished plates of rocksalt, on which no trace of visible moisture is deposited, will be conceded to me. This tube involves the use of a source of heat of small dimensions. The lime light, though fulfilling this condition, is not suitable, because of the high refrangibility of its heat. The incandescent platinum spiral would be better; still, the radiation from this source is but feebly absorbed by the aqueous vapour taken up by air at ordinary temperatures. The oxyhydrogen flame fulfils the required conditions best.* It has the advantage of high temperature and low refrangibility, while the fact that its heat, as coming from aqueous vapour, is absorbed with special energy, by aqueous vapour, is also a strong recommendation. In the Bakerian Lecture for 1864, I have illustrated this point. It was then shown that when a platinum spiral, rendered incandescent by a voltaic current, had 5·8 per cent. of its heat absorbed by un-dried air, a hydrogen flame had from 17 to 20 per cent. of its heat absorbed. The mere plunging of a platinum spiral into the flame caused the absorption to fall from 17 per cent. to 8·6 per cent. Hence my reason for choosing a hydrogen flame in the present instance.

Dry air and humid air being caused to occupy the experimental tube in succession, both of them were compared with the radiation through the tube when very perfectly exhausted by a BIANCHI's pump. The following results were obtained.

	Deflection.	Absorption per 100.
Vacuum	0·0°	0·0
Dry air.	0·0°	0·0
Humid air.	8·0°	11·7
Vacuum	0·0°	0·0
Dry air.	0·0°	0·0
Humid air.	7·6°	11·2

The deflection through a vacuum, otherwise the total heat, was 47° which, according to my calibration table, is equivalent to 68 units.

A fresh supply of hydrogen was here introduced into the gas holder. The total heat being 46° or 65 units, the following results were obtained.

	Deflection.	Absorption per 100.
Dry air.	0·0°	0·0
Humid air.	6·2°	9·5
Dry air.	0·0°	0·0
Humid air.	6·7°	10·3

Mean of the four determinations with humid air 10·7.

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* I was careful to assure myself that, unless it amounted to the visible wetting of the plates of salt, there was no sensible stoppage of the rays from the oxyhydrogen flame. This quite agrees with the result obtained by MAGNUS himself, in the experiment with the concave mirror already referred to. Unless he visibly wetted the mirror he failed to impair the energy of the reflected beam.

A column of humid air 38 inches long absorbs, according to these experiments, 10·7 per cent. of the radiation from a hydrogen flame. I have been criticised for estimating the absorption of the earth's rays within 10 feet of the earth's surface at 10 per cent. This estimate I consider a moderate one, and the foregoing experiments prove it to be so.

It would be an error to suppose that determinations like these are easily made. They require the most scrupulous care for their successful accomplishment. The hydrogen issued from the gas holder through an orifice of fixed dimensions in a stream of the utmost possible constancy. It was then led into a SUGG's regulator, whence it issued under an absolutely constant pressure. The flame issued from a circular brass burner with an aperture $\frac{3}{16}$ths of an inch in diameter. It was carefully surrounded by a hoarding, the space within the hoarding being packed with horsehair. Every precaution, in fact, was taken to avoid the agitation of the air around the flame. Proper care was also taken to secure the pile against disturbance by air currents. The air being first purified, by passing it through caustic potash and sulphuric acid, was rendered humid by carrying it over wet bibulous paper contained in a suitable tube. It required some minutes to enter, and it was therefore necessary, by prior patient observance of the needle, to make sure that during this interval no change occurred in the radiation save that effected by the humid air itself. The humid air was removed from the experimental tube, not by exhaustion which always causes precipitation, but by gently forcing, by means of a compressing pump, dry air through the tube. When this was done, the needle, in all the experiments above recorded, returned within a small fraction of a degree to zero.*

With the rough, wide experimental tube to which reference has already been made, I, ten years ago, found the absorption of a column of humid air 38 inches long, to be 8 per cent. of the total radiation from a flame of hydrogen.

§. 6. *Conservation of Molecular Action.*

If the absorption of radiant heat be the act of the constituent atoms of compound molecules, its amount depending solely on the number of molecules encountered by the calorific waves, then whatever may be the changes of density which gases and vapours undergo, so long as the number of molecules remains the same, the absorption ought to continue constant. Such constancy, should it be proved to exist, I name the "conservation of molecular action." The experiments now to be described deal with this question.

Besides the silvered experimental tube already described as 38 inches long, I had another constructed of the same diameter, and with similar terminal apertures. Its length was 10·8 inches. The one tube was, therefore, 3·5 times the length of the other. The shorter tube was constructed with the view of proving that the absorptions I had recorded in previous memoirs were exerted by the vapour molecules, and not by liquid layers deposited on my plates of salt. The hypothesis of such layers being, however, completely disposed of, we can carry the experiments a step further. Assuming that the absorption does not change with change of density, so long as the quantity remains constant, it would follow that 1 mercury inch of vapour in the long tube ought to quench as much heat as 3·5 inches in the short one. The same conclusion ought, of course, to hold good when we compare 2 inches of vapour in the long tube with 7 inches in the short one.

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* The temperature of the laboratory air during the foregoing experiments was 60° FAHR.

The experiments have been made, and with the following results :—

SULPHURIC ether vapour.

SHORT experimental tube.

Pressure.	Deflection.	Absorption per 100.
3·5 inches . . .	24°	30
7 „ . .	30°	37·5
Total heat .	50°	

Again :—

Pressure.	Deflection.	Absorption per 100.
3·5 inches . . .	24°	30
7 „ . . .	31·5°	39·4
Total heat .	50°	

Taking the means of these two experiments we have the absorptions—

For 3·5 inches	30 per cent.
For 7 „	38·5 „

Such is the result obtained with the short tube : we now turn to the long one.

LONG experimental tube.

Pressure.	Deflection.	Absorption per cent.
1 inch	23°	30·3
Total heat .	49°	
2 inches . . .	31°	38·8
Total heat .	50°	

These results are almost identical with those obtained with the short tube and greater pressures.

The source of heat in this case was a dull lime light. In subsequent experiments the light was brightened. Here is the result.

LONG experimental tube.

Pressure.	Absorption.
1 inch	22·3 per cent.
2 inches	29·5 „

SHORT experimental tube.

Pressure.	Absorption.
3·5 inches	22·5 per cent.
7·0 „	30·0 „

The agreement here is almost as close as that established by the first experiment. With a still brighter source of heat the absorptions were :—

LONG tube.

1 inch	18·4 per cent.
2 inches	25·7 „

SHORT tube.

3·5 inches	18·8 per cent.
7·0 „	25·6 „

When, therefore, the density of the vapour varies inversely as the length of it traversed by the calorific rays, the absorption remains constant.

The hydride of amyl lends itself conveniently to experiments of this character. With it, and with the lime light as source, the following measurements have been made :—

HYDRIDE of amyl.

LONG experimental tube.

Pressure.	Deflection.	Absorption.
1 inch	10°	12·8
2 inches . . .	15°	19·2
Total heat	49·4°	

SHORT experimental tube.

Pressure.	Deflection.	Absorption.
3·5 inches . . .	10°	12·2
7·0 „ . . .	15°	18·3
Total heat	50·5°	

The agreement here is close enough to illustrate the law; the greatest difference being under 1 per cent.

At this point the following entry appears in my note book:—"It might have been wise on my part to rest content with the comparison of the long and short tubes with the lime light as source of heat. But for the sake of completeness I wished to introduce the incandescent spiral. The fulfilment of this wish—that is to say, the successful performance of a single experiment—has cost me more than a week's work. Once however committed to the task, I could not leave it incomplete.

"The discrepancy between the two tubes was in no case great, hardly ever exceeding 2 per cent. But the difference was uniformly in favour of the long tube and small density. Diaphragms were employed, the position of the short tube was shifted, and it was finally placed so that the pile should occupy the same position in relation to its adjacent end as it did in relation to the adjacent end of the long tube. The discrepancies then disappeared, the absorptions in the two tubes proving practically identical.

"Many leaves of paper were covered with observations during the week, but it is useless to take up time and space in copying them here. One representative observation will suffice."

SULPHURIC ether.

Source of heat—bright red spiral with rocksalt lens.

LONG experimental tube.

Pressure.	Deflection.	Absorption.
1 inch	20·0°	23·5
2 inches . . .	27·3°	32·1
Total heat	51·0°	

SHORT experimental tube.

Pressure.	Deflection.	Absorption.
3·5 inches . . .	17·8°	23·4
7·0 „ . . .	24·8°	32·6
Total heat	49·0°	

The agreement between the two tubes is as perfect as could be desired.

It is easy to record these experiments, but it is not so easy to make them. On every portion of the apparatus—the source, the tube, the thermopile, and the galvanometer —extraordinary care must be bestowed to make the experiments strictly comparable. The results were checked by taking the total heat after every experiment—a precaution which insured the detection of any variation on the part of the source. When the platinum spiral was used, the battery of ten GROVE's cells, employed to render it incandescent, had to be very carefully prepared, freshly amalgamated zinc plates being used in each fresh battery. The requisite deflection of a tangent compass being produced, it was kept constant by means of a rheochord throughout the day. When the battery showed signs of rapid falling it was always renewed. It would be tedious to dwell upon the precautions taken to protect the source and the pile from the least agitation of the air. Such precautions are essential, but their necessity and form must be learnt by each experimenter for himself.

§. 7. *Thermal Continuity of Liquids and Vapours.*

I have amply illustrated by experiments, recently made, the correspondence which subsists between vapour absorption and liquid absorption, when the quantities of matter traversed in the two cases by the calorific rays are proportional to each other. This correspondence, as I have already stated, was established eighteen years ago. And though the result goes to the very core of the discussion which my researches have aroused, though in relation to that discussion they had, in my estimation, a weight and import greater than those of any other experiments published by me, they seem never for a moment to have attracted the attention of those who have taken part in the discussion. Here is a result, published in 1864, which illustrates the point now under consideration:—

		Absorption per 100.	
		Vapour.	Liquid.
Bisulphide of Carbon	0·48	4·3	8·4
Chloroform	0·36	6·6	25·0
Iodide of Methyl	0·46	10·2	46·5
Iodide of Ethyl	0·36	15·0	50·7
Benzol	0·32	16·8	55·7
Amylene	0·26	19·0	65·2
Sulphuric Ether	0·28	21·5	73·5
Acetic Ether	0·29	22·2	74·0
Formic Ether	0·36	22·5	76·3
Alcohol	0·50	22·7	78·6

The magnitude of the absorption in the liquids is far greater than in the vapours; because the quantity of absorbent matter is far greater in the former than in the latter; but the order of absorption is the same.

When the vapours are doubled in quantity the absorptions are considerably increased. When trebled they are still further augmented; in other words, they approach more and more in magnitude to the absorptions of the liquids; but the harmony as regards order is never disturbed. What then would occur if the vapours were so increased as to render the quantities of matter in the two states, not proportional, but *equal* to one another? This is the question with which I now propose to deal. At the time when the results above recorded were obtained, I thought it probable that if a circular liquid layer of a given diameter could be vaporized in a tube of the same diameter, the absorption would remain unchanged. In other words, I thought that the liberation of the molecules from liquid cohesion would neither augment nor diminish their action upon radiant heat. Since 1864 this problem has been often in my mind. The wide silvered tube, I am happy to say, has rendered the solution of the problem possible.

It is only highly volatile liquids that lend themselves to this experiment, because from them alone can vapours be derived of sufficient density to produce liquid layers of practicable thickness. On the 22nd of last October the experiment was first attempted. The source of heat was the lime light, the rays of which were received by a concave mirror silvered in front, and sent in a nearly parallel beam through the experimental tube. At the end nearest the source the tube was provided with a diaphragm having a circular orifice 1 inch in diameter. At the other end was a diaphragm with an orifice ½ an inch in diameter. Beyond this was placed the thermopile furnished, not with its reflecting cone, but with a tube of brass (shown in fig. 3) 2 inches long, and blackened within. In this arrangement, the heat which reached the pile did not even approach the interior cylindrical surface. The total heat employed produced a deflection of 60 galvanometric degrees, which, when the tube was exhausted by a powerful BIANCHI's air-pump, was accurately neutralised by a compensating cube. Liquid sulphuric ether was then placed in a large flask provided with a sound stopcock, the object being to expose a considerable evaporating surface. The flask was plunged in water, with the view of keeping the liquid and its vapour at an approximately constant temperature. The air being carefully removed from the flask, it was attached to the experimental tube, and a quantity of vapour was allowed to enter sufficient to render a column 38 inches long equivalent to a liquid layer 1 millimeter in thickness. Two concurrent experiments made the deflection produced by the vapour

41°

Without altering the quality of the heat, the absorption exercised by a liquid layer of sulphuric ether 1 millimeter thick was next determined. The rocksalt cell with which the experiment was made is described in detail in the Bakerian Lecture for 1864 (Phil. Trans., Vol. 154, p. 328.) The annexed figure (fig. 5) will give a sufficiently clear notion of its construction and disposition. Between two stout plates of brass, *c c*, and its fellow, two rocksalt plates of extreme purity are firmly clasped by suitable screws, due care being taken to protect the plates from a crushing pressure. The two brass plates referred to are perforated by circular apertures as shown in the figure. The two plates of salt are not allowed to come into contact but are separated from each other by a carefully worked brass plate 1 millimeter thick, perforated like *c c*. A portion of this middle plate is cut away, opening a passage into the interior of the cell. Through this passage the cell is filled with liquid by means of the funnel *f*. The cell is placed on a platform P P riveted on to the double brass screen S S. The source was placed at A, and the thermopile with its blackened tube at B. It is not necessary to figure them.

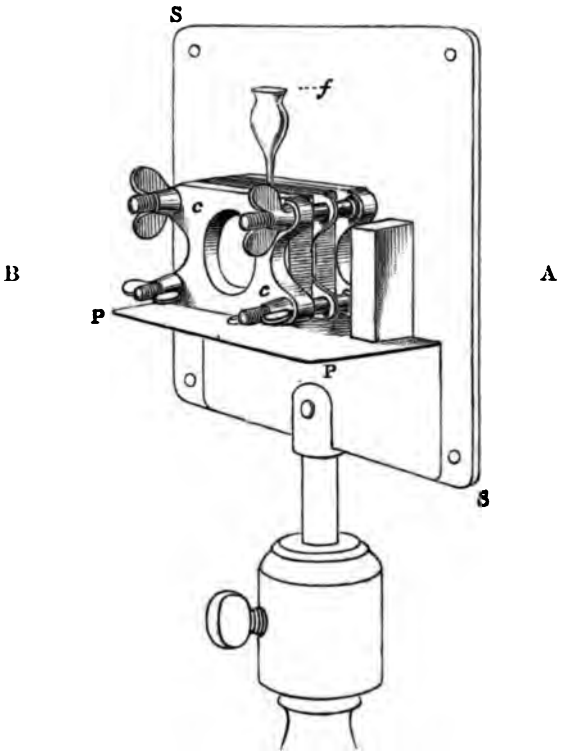
The needle being brought accurately to zero by the compensating cube when the cell was empty, liquid sulphuric ether was poured in. The consequent deflection was

42°

Here vapour absorption and liquid absorption were so nearly alike as to stimulate me to further efforts. The entry in my note book on the 22nd ends with the remark: "I purpose repeating this experiment on Monday, with the view of rendering the result secure."

On Monday, accordingly, the experiments were resumed, but they proved by no means so easy as I had hoped to find them. For five days I worked at the subject without coming to any satisfactory conclusion. The source, the mirror, the cell, the experimental tube, and the thermopile, were all in turn objects of scrutiny; but there still remained a difference between the action of the liquid and that of its vapour sufficient to throw doubt on the assertion of their identity.

Fig. 5.



At the conclusion of many trials and precautions I found the absorption of the vapour still distinctly in excess of that of the liquid. I had reduced the spherical aberration to a minimum, by confining the reflection to a small central area of the silvered mirror. Still the image of the incandescent lime, formed at the end T′ of the experimental tube, was large enough to encroach a little on the annular space surrounding the aperture closed by the plate of rocksalt. Diaphragms of polished metal had also been used to lessen the amount of heat falling upon the pile. I figured to myself the heat impinging on the annular space and diaphragm, reverberated back to the end T of the experimental tube, reflected from the annular space and diaphragm at that end, and thus in part sent back to the pile. Such heat, instead of passing once through the vapour, would pass through it three times, and if it formed a sensible part of the total heat, might make vapour absorption appear greater than liquid absorption. I had the tube dismounted, and the annular spaces and diaphragms carefully coated with lampblack. Remounting the tube and measuring once more the vapour absorption, it was found to be

32·4 per cent.

This was the mean of five concurrent series of observations, in which every care was taken to ensure exactitude. Lest the total heat should vary, during the execution of a series, it was taken at the conclusion of every individual experiment.

The absorption of sulphuric ether, acting as a liquid, was next determined. The mean of three series of experiments, two of which yielded identical results, and the third of which differed only 0·7 per cent. from the others, made the absorption of the liquid ether to be

32·9 per cent.

which, I need not say, is surprisingly close to the vapour absorption, differing therefrom by only 0·5 per cent.

Informed by experiment that the heat from the incandescent platinum spiral was more powerfully absorbed than that of the lime light, I thought it worth while to inquire whether the liquid followed its vapour in absorptive energy, when the quality of the heat was changed. On Monday the 31st, accordingly, the rays from the spiral being rendered parallel by a rocksalt lens, the absorption of sulphuric ether was determined and found to be

66·7 per cent.,

while the absorption of the liquid was

67·2 per cent.

which again gives a difference of only 0·5 per cent.

On another occasion I found the absorption by sulphuric ether to be:

Vapour	71 per cent.
Liquid	70 „

On the 1st November I checked the result obtained with the lime light and mirror, by using the lime light and rocksalt lens. Here are the absorptions of the vapour and liquid respectively:—

Vapour	33·3 per cent.
Liquid	33·3 „

The absorptions are identical; while the result agrees closely with that obtained with the concave reflector.

When the requisite quantity of sulphuric ether vapour, viz. 7·2 mercury inches, was in the experimental tube, I tried whether the radiation from a LESLIE's cube, coated with lampblack and filled with boiling water, could pass through the vapour. About 14 per cent. of the incident heat was transmitted. Had I been asked at the time whether a liquid layer of sulphuric ether 1 millimeter thick was pervious to the heat of the cube, I should have replied with some confidence in the negative. Hence, for the moment, I thought the experiment opposed to the law that vapour absorption and liquid absorption, when equal quantities of matter are compared, are the same. On actually testing a layer of the liquid ether 1 millimeter thick, the transmission of upwards of 6 per cent. of the incident heat was observed. So that in this case also we have harmony of deportment between liquid and vapour. The absorption of the vapour exceeds that of the liquid because the heat from the cube radiated freely against the interior silvered surface of the experimental tube, and by its reflection from that surface had its path through the vapour augmented in length. This augmentation naturally carried with it an increase of the absorption.

The next substance examined was hydride of amyl, the boiling point of which is 30° FAHR., or 5° lower than that of sulphuric ether. When a large evaporating surface is exposed, there is therefore no difficulty in obtaining, from this liquid, vapour of a pressure of 6·6 inches of mercury. This, in a tube 38 inches long, would, if squeezed to liquefaction, produce a layer 1 millimeter thick. Vapour absorption and liquid absorption being measured in succession, this is the behaviour of the hydride of amyl:—

Absorption by vapour	51 per cent.
Absorption by liquid	51 per cent.

the two absorptions being absolutely identical.*

Combining this section and the last, their joint results may be thus summed up. Beginning with a column of sulphuric ether vapour 38 inches long at 7·2 inches pressure, or with a column of hydride of amyl vapour 38 inches long and at 6·6 inches pressure, and gradually shortening the column without altering the quantity, the vapour would gradually augment in density and pass wholly, when reduced to a thickness of 1 millimeter, into the liquid state of aggregation. Suppose a beam of heat of constant value, after passing through the vapour, to impinge upon a thermopile and to produce a definite galvanometric deflection; this deflection would remain absolutely fixed during all the changes of density and aggregation which we have supposed the vapour to undergo. In other words—as regards the absorption of radiant heat, the vapour would pass, without breach of continuity, through all its stages of condensation into the liquid form of matter.

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* When the rocksalt cell was empty, reflection of course occurred at its two interior surfaces. A perfectly diathermanous liquid, with the refractive index of rocksalt, would annul this reflection. And though the liquids actually employed had a smaller refractive index than rocksalt, and though they were far from being perfectly diathermanous, their introduction into the cell must nevertheless have diminished the reflection and thus added to the transmitted heat. This addition, having been determined by calculation, was sensibly neutralised by the introduction of washers of thin note paper which slightly augmented the thickness of the liquid stratum traversed by the calorific rays.

A general law of molecular physics is, I apprehend, here illustrated.

§. 8. *Rhythmic Absorption of Radiant Heat by Gases and Vapours.*

Conclusive as the foregoing experimental argument must appear, as regards the action of free molecules upon radiant heat, I am nevertheless glad to supplement it by another of a totally different character. On the 29th of November, 1880, I had the pleasure of witnessing, in the laboratory of the Royal Institution, the remarkable experiments of Mr. GRAHAM BELL, wherein musical sounds were evoked by causing an intermittent beam of light to impinge upon thin discs of various kinds of matter. I was soon convinced that the effects were due to the rhythmic gain and loss of heat. Being occupied with experiments on gases and vapours at the time, I thought that they might be invoked to decide the nature of the action revealed by Mr. BELL. The result was mentally clear before the experiment was made. I pictured a highly absorbent vapour exposed to the shocks of the intermittent beam, suddenly expanding during the moment of exposure, and as suddenly contracting when the beam was intercepted. Pulses of an amplitude probably far greater than those obtainable with solids would, I thought, be thus produced; and these pulses, if caused to succeed each other with sufficient rapidity, would be sure to produce musical sounds.

This idea was tested and verified on the spot. The Journal of Telegraph Engineers for December 8th, 1880, contains the following record of what occurred. "When Professor BELL was good enough to show me his experiments, I happened to be myself experimenting on the action of vapours upon radiant heat. Old experiments had revealed, and new ones had confirmed the fact that, as regards the absorption of heat, there existed vast differences between vapours. This is well illustrated by the deportment of bisulphide of carbon and of sulphuric ether, one of which is highly transparent, and the other highly opaque to radiant heat. It occurred to me that, if the action were due to the absorption of heat, we might possibly extract musical sounds from sulphuric ether vapour, whereas bisulphide of carbon vapour being transparent to heat-rays they would for the most part go through this vapour unabsorbed, and produce no sonorous effect. I think Professor BELL will bear me witness as to the result. We placed a quantity of sulphuric ether vapour in a test tube, and allowed an intermittent beam of light to strike upon the vapour far above the liquid, and we heard distinctly a musical tone of a pitch corresponding to the rapidity of the flashes. We then took the bisulphide of carbon vapour, and tried it in a similar manner, but neither Professor BELL nor myself could hear any trace of a musical sound."*

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* Mr. BELL has given a perfectly accurate account of this occurrence in the Philosophical Magazine,

It was obvious, however, that the arrangement of Mr. BELL—a truly beautiful one—was not suited to bring out the maximum effect. He had employed a series of glass lenses to concentrate his beam, and these, however pure, would, in the case of transparent gases, absorb a large portion of the rays most influential in producing sound. This may be illustrated by comparing a rocksalt lens, in my collection, with a glass lens of the same focal length prepared in the workshop of M. DUBOSCQ. Transmitted through the former, the radiation from an incandescent platinum spiral produced a galvanometric deflection of 55°, possessing, according to the table of calibration, a value of more than 100. Transmitted through the latter, the deflection fell to 10°, or to less than $\frac{1}{10}$th of the radiation transmitted by the rocksalt. The $\frac{9}{10}$ths here shown to be intercepted by the transparent glass, consist of heat on which transparent gases and vapours would exert a specially absorbent power. Hence the desirability of maintaining this important factor, in the radiation employed to test the sonorous power of such substances.

It was with the view of preserving intact these powerful calorific rays that I employed in my experiments on calorescence* small concave mirrors silvered in front; and to these mirrors I now resorted. My more intense sources of heat comprised a SIEMENS' lamp connected with a dynamo machine; an ordinary electric lamp connected with a voltaic battery; and a lime light, produced sometimes by the combustion of oxygen and hydrogen, and sometimes by oxygen and coal gas. The lime light (which was used by me in 1859) is so handy, steady, and otherwise effective, that I have applied it almost exclusively throughout this part of the inquiry. Sources of heat, however, of much lower temperature than the lime light, have proved competent to evoke musical sounds. A candle flame, a red hot coal, a red hot poker, the same poker at the temperature of boiling water, and an incandescent platinum spiral, have all been proved effective, though of course far less so than the concentrated lime light.†

To produce the required intermittence I first employed a circle of sheet zinc 16 inches in diameter provided with radial slits. This was afterwards exchanged for a second disc of the same diameter, but furnished with circumferential teeth and interspaces. The disc was mounted vertically on a whirling table, and caused to rotate across the beam near the focus of the concave mirror. Immediately behind the disc was placed the flask containing the gas or vapour to be examined, while an india-

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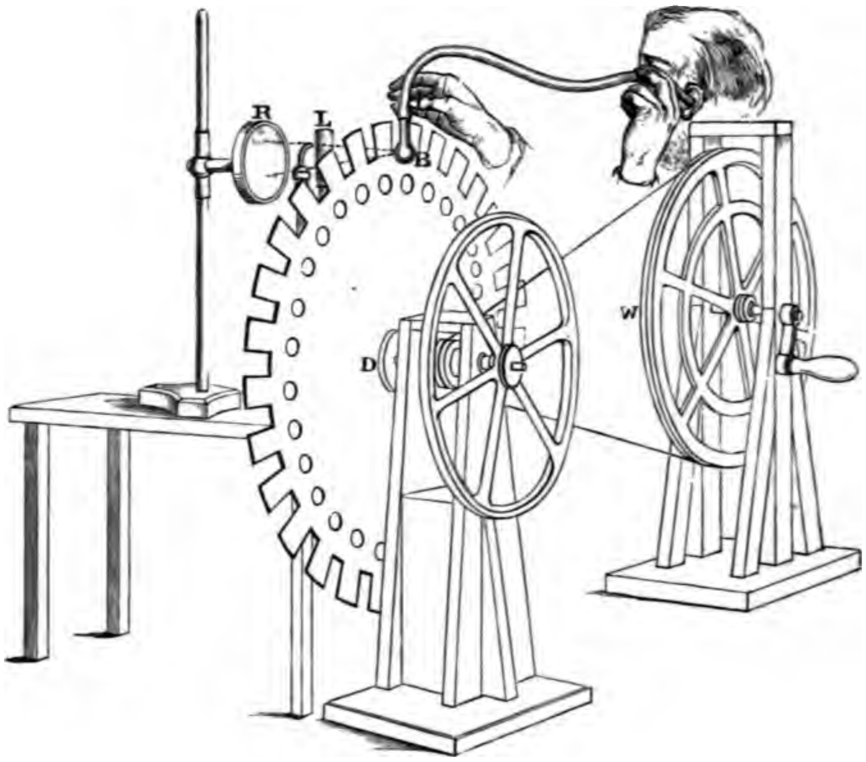
vol. xi., p. 519. With reference to what occurred on the 29th of November he writes thus:—"Professor TYNDALL at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapour of sulphuric ether (a good absorbent of heat), and another with the vapour of bisulphide of carbon (a poor absorbent); and he predicted that, if any sound was heard, it would be louder in the former case than in the latter. The experiment was immediately made; and the result verified the prediction."

* Philosophical Transactions, 1866, Vol. 156, p. 1.

† These earlier experiments will be found recorded in the Proceedings of the Royal Society, vol. 31, pp. 307, 478.

rubber tube, ending in a hollow cone of ivory or boxwood, connected the flask with the ear. With this arrangement, simple as it is, sounds of surprising intensity were obtained with all those gases and vapours which my previous experiments with the experimental tube and thermopile had proved to be powerful absorbers of radiant heat. The final arrangement was that shown in fig. 6.

Fig. 6.



The source of heat is the carefully worked and centred lime-cylinder L, heated by the oxy-hydrogen flame. The rays from this source are received by the concave mirror R, and converged upon the bulb B which contains the substance to be tested. The bulb is connected with the ear by a piece of india-rubber tubing, ending in a tapering tube of boxwood or ivory. The intermittence of the calorific beam is effected by the disc D of strong cardboard, 2 feet in diameter, and provided at the circumference with 29 teeth and corresponding interspaces.* The disc is caused to rotate by the wheel W, with which it is connected by a band. The positions of the sonorous bulb and of its ear-tube are shown in the figure. In the case of gases lighter than air the bulb B is turned upside down. With the heavier gases it is held erect. When vapours are tested, a small quantity of liquid is poured into the bulb, which is shaken so as to diffuse the vapour in the air above the liquid. The bulb is held so that the point of maximum concentration of the beam falls upon it.

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* Intermittence is sometimes produced by the series of equidistant circular apertures shown in the figure.

With this apparatus I have tested more than once the sounding power of ten gases and of about eighty vapours. As a sound producer chloride of methyl is supreme. It is however closely followed by aldehyde, olefiant gas and sulphuric ether, the two latter being very nearly equal to each other. The volatility of the liquid from which the vapour is derived is of course an important factor in the result. For, however high the inherent capacity of the molecule as an absorber may be, if the molecules be scanty in number the effect is small. Feeble vapours, on the other hand, may to some extent atone by quantity for the inherent weakness of their molecules. A few examples will suffice to show how the specific action of the molecules over-rides the effect of volatility. Bisulphide of carbon with a boiling point of 43° C. is less powerful than acetic ether with a boiling point of 74°. Tetrachloride of carbon boils at 77° but its sound by no means equals that of acetal which boils at 104°. Chloroform with a boiling point of 61° is less powerful as a sound producer than valeral with a boiling point of 100°, or even than valerianic ether with a boiling point of 144°. Cyanide of methyl boils at 82°, but produces less sound than acetate of propyl with a boiling point of 102°. In the experimental tube, these vapours follow, as absorbers, the order of their sounds. When tested in liquid layers they follow the same order. I have examined about a score of liquids with boiling points varying from 163° to 308°. At ordinary temperatures the vapours of these liquids were practically inaudible. The liquids being plunged in a bath of heated oil the vapours so produced emitted, for the most part, powerful sounds. The measured absorptions of a sufficient number of substances, in relation to their sounding power, shall be tabulated immediately.

The fact is worth a passing reference that the chlorides of the elements appear one and all to be feeble sound producers, because they are one and all feeble absorbers of radiant heat. Many years ago I had found them highly diathermanous, and accepted chloride of sodium as representative of the class. Silicium chloride, for example, though very volatile is weak as a sound producer. Tetrachloride of carbon, and terchloride of phosphorus are also volatile, but not strong. Bichloride of tin, chloride of arsenic, chloride of titanium, and chloride of sulphur, are all feeble sound producers. In these three cases the boiling points are high, but non-volatility is not the cause of the weakness, for when the vapours are raised, by heating their liquids, almost to the pressure of the atmosphere, they are still but feebly sonorous. Whatever then the condition may be which renders these substances pervious to radiant heat, it appears to be a condition common to them all.

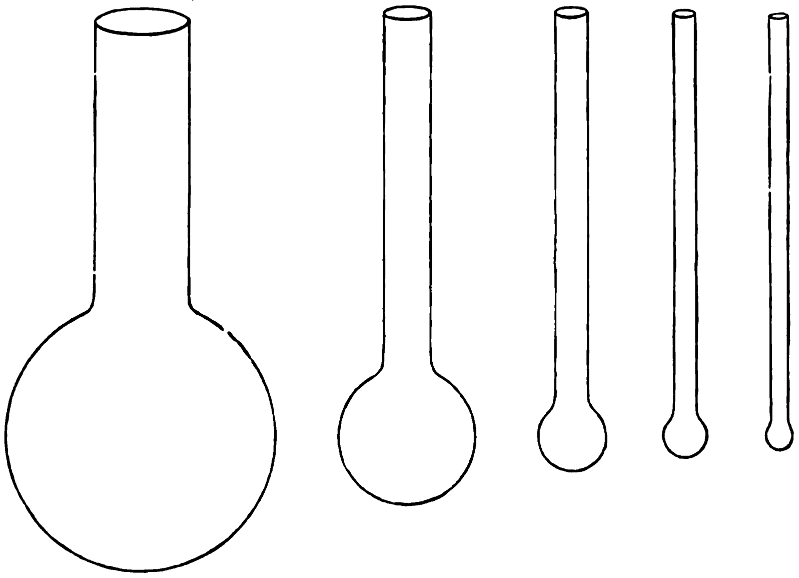
In experiments with the chlorides care must be taken to abolish all fumes. Bichloride of tin sounds loudly with fumes, but is weak without them. Simply heating the upper part of the flask frequently suffices to lower the sounds from loudness almost to silence.

The universality of its presence, and the discussions which it has provoked, rendered the action of water vapour especially interesting to me. I did not imagine at the outset that the modicum of vapour diffused in atmospheric air at ordinary temperatures could produce sonorous pulses of sensible intensity. In my first experiment, therefore, I warmed water in a flask nearly to its boiling point. I heated the flask above the water with the spirit lamp flame, thus dissipating every trace of haze, and then exposed the clear vapour to the intermittent beam. The experiment was a virtual question put to the vapour as to whether I was right or wrong in ascribing to it the power of absorbing radiant heat. The vapour answered by emitting a musical note which, when properly converged upon the tympanum, seemed as loud as the peal of an organ. When the temperature was lowered from 100° C. to 10° C., the sound did not vanish, as I expected it would. It remained not only distinct but strong. The flasks employed in these experiments were dried in a variety of ways, of which I have already given some account, and which will suggest themselves to every experimenter in this field. Taken open from the laboratory, and exposed to the intermittent beam, the flasks are always to some extent sonorous. Placed beside sulphuric acid underneath the receiver of an air-pump, and permitted to dry there, they are reduced to silence. The slightest invasion of humid air renders them again sonorous. Breathing for a moment into a dried and silent flask, a loud sounding power is immediately manifested.

Flasks without lips have been specially blown for these experiments, the india-rubber tubing being readily pushed over their necks. Large flasks are not the most suitable. To produce effective pulses sudden and intense expansions and contractions are required, and these are best obtained when the beam, at its place of maximum concentration, covers a large portion of the matter in the flask. Thin bulbs about a cubic inch in volume are both handy and effective ; but the bulb may be reduced to $\frac{1}{50}$th, or even $\frac{1}{100}$th of a cubic inch, without rendering the sound insensible. A speck of water introduced into such tiny bulbs, when vaporised by heat, produces sounds which are not only sensible but loud. A series of bulbs which I have actually employed in my experiments are represented in their natural dimensions in fig. 7.

It cannot be necessary to state that the absorption which produces the pulses is direct and immediate, being the act of the gaseous molecules. The pulses are not due to the heating of the glass envelope and the communication of its heat to what it contains. For, were this the mode of heating, air would be as sonorous as olefiant gas. Nor are the pulses due to the sudden vaporization of a liquid layer which might be supposed to overspread the interior of the flask. When water at a low temperature is purposely caused to cover the interior surface, exposure to the beam produces sound of a certain intensity. When the flask is so heated in a spirit flame as to chase away every trace of the adherent liquid, the exposure of the pure vapour, then within the flask, to the beam, generates a sound far louder than that produced when the liquid film was there. Holding the bulb containing the hot vapour for a little time in the intermittent beam, its temperature falls; the quantity of vapour diminishes, and the sound sinks in intensity. On quitting the spirit flame, the bulb in some cases must have been near a red heat, but even at this temperature the vapour sounded loud.

Fig. 7.



It has, I think, been amply shown that when the molecules of a liquid are rendered free by vaporization they carry with them their absorbent power, liquids and vapours being pervious and impervious to the same quality of heat. Hence the inference, that prior transmission through a liquid of sufficient thickness ought so to sift a calorific beam as to render it powerless to act on the vapour of that liquid. Even with the loudest sounding vapours this proves to be the case, a layer of liquid $\frac{1}{8}$th of an inch thick being found generally sufficient to deprive the beam of its efficient rays, and the vapour of its sounding power.

In transparent liquids, the visible rays have free transmission, the destruction of sounding power by such liquids must, therefore, be due to the absorption of the invisible calorific rays. This induction, which hardly needs verification, is nevertheless capable thereof. Many years ago I pointed out the astonishing transparency of dissolved iodine to the invisible heat rays. Placed in the path of the intermittent beam, a layer of this substance, perfectly opaque to light, does not sensibly diminish the sound of transparent gases and vapours. To such substances, the iodine is exactly complementary, arresting the rays which they transmit, transmitting the rays which they absorb, and, therefore, not interfering with the sounding power.

That sounds may also be produced by the absorption of the visible rays is well exemplified by the deportment of iodine and bromine vapours, both of which yield with the lime light forcible sounds. Here the intervention of a transparent liquid, however adiathermanous it may be, produces no sensible effect upon the sound, the reason being that it permits the particular rays which act upon the coloured vapours to pass freely through it. A layer of dissolved iodine, on the other hand, deprives the beam of its power of evoking sounds from either iodine or bromine vapour.

The rotation which produces the maximum effect is soon ascertained by experiment. The sound is loudest when the pulses succeed each other in periods which invoke the resonance of the flask. I possess a hollow cone with a well-polished rocksalt base, by which this point is well illustrated. Filling this cone with chloride of methyl, while the base is turned towards the source of heat, the apex of the cone being connected by a tube with the ear, sounds of extraordinary intensity are produced. Abandoning the ear-tube, the sound can be heard at a distance. But to obtain this effect the speed of rotation must be definite and constant. The maximum sound once obtained, either the lowering or the heightening of the speed rapidly enfeebles it. It is difficult by hand-turning to keep the rate of rotation constant. Hence the desirability of a mechanical arrangement, which would ensure the proper rapidity and necessary uniformity. One or two motive powers have been tried, including a small steam-engine and an electro-magnetic engine, but the arrangement has not yet been brought to perfection.

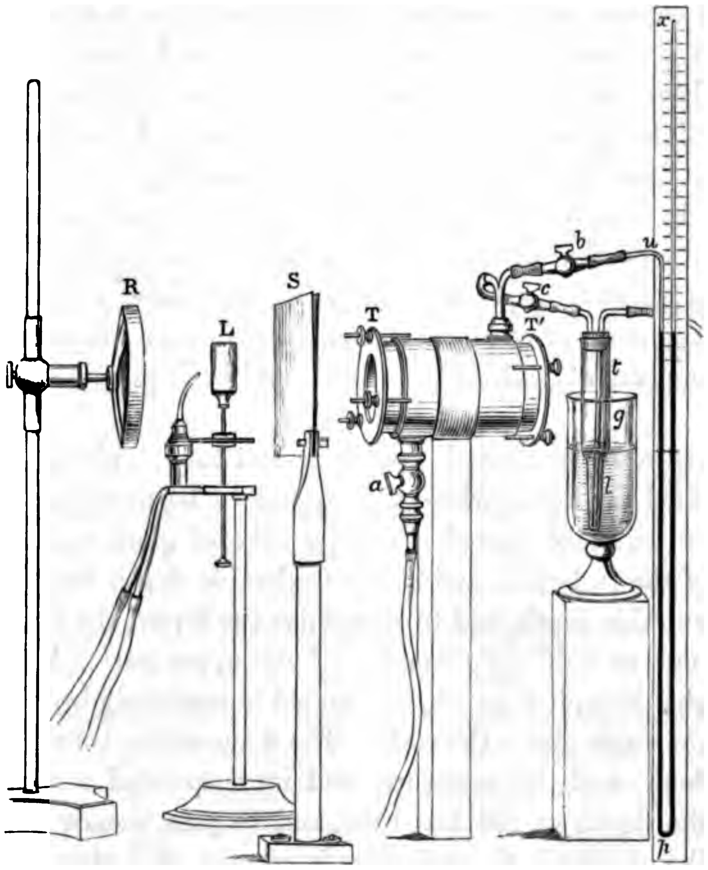
§. 9. *Manometric measurements.*

Some time before the visit of Mr. GRAHAM BELL in November, 1880, I had inserted into my old experimental brass cylinder a narrow tube of glass, which being bent at a right angle a few inches above the cylinder could hold an index of coloured liquid in its horizontal portion. I had long known that the absorption of radiant heat must be accompanied by the expansion of the absorbing body, but thought that such expansion would furnish only a rough measure of the absorption. With ordinary sources of heat I found the expansion small, even when sulphuric ether occupied the experimental tube; but when a pair of stout carbons, rendered incandescent by a SIEMENS' machine, were employed as a source, the liquid index was driven forcibly out of the narrow glass tube.

The experimental tube, however, was but a rude manometer, and I therefore sketched and described to my assistant at the time, with a view to its construction, a handier instrument. The apparatus was to consist of a short tube with rocksalt ends, capable of being exhausted and filled with any required gas or vapour. Through this tube it was proposed to send a concentrated calorific beam, whose action on an absorbent gas or vapour should be declared by the depression of a liquid column in one leg of a U tube, and its elevation in the other. Two rocksalt plates were to be employed with the view of allowing the beam free escape from the tube after it had done its work upon the gas or vapour. The warming of the apparatus by the reverberation of the heat would be thus avoided. The point aimed at was to effect the expansion of the gaseous body purely by radiant heat, and undisturbed as far as possible by heat derived from the envelope.*

A number of manometric tubes of different lengths and materials were constructed on this principle, some being of glass and some of metal. The instrument with which the measurements now to be recorded were executed is represented in fig. 8. T T′ is a glass tube 4 inches long and 3 inches in diameter. It is provided with brass flanges at the ends which reduce the diameter to 2·5 inches. Against these flanges, transparent plates of rocksalt are fixed air-tight. The tightness of the tube was secured, sometimes by india-rubber washers properly greased, and sometimes by cement.† A stop-cock *a*

Fig. 8.



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* Professor Röntgen was, I believe, the first to turn the expansion of gases to account in demonstrating the absorption of radiant heat. The very day, moreover, on which I made my communication to the Society of Telegraph Engineers, viz. the 8th of December, 1880, he forwarded to a scientific journal the announcement of his having obtained sounds from coal-gas and ammonia (see Wiedemann's Annalen, Jan. 1881). His subsequent experiments with aqueous vapour, &c., agree with mine.

† The absorption of the vapours by india-rubber—which was in some cases extraordinary—caused the washers to be abandoned.

near one end of T T′ was connected with a barometer tube and an air-pump. A T-piece at the other end was connected on the one side with a purifying apparatus (not shown), consisting of two U tubes, one containing fragments of Carrara marble wetted with caustic potash, the other containing fragments of glass wetted with sulphuric acid. Before entering these U tubes the air was freed from suspended matter by a plug of cotton wool. On the other side the T-piece was connected with a quill tube of glass bent into the shape of a U, in the two legs of which a coloured liquid stood at the same level. The liquid column when standing at the same level in both arms of the U was 350 millimeters high in each, while the free leg of the U rose to a height of about 500 millimeters above the surface of the liquid (shortened in the figure). The source of heat was the lime cylinder L, rendered incandescent by a flame of coal gas and oxygen. The rays from the lime cylinder were received by a concave mirror R silvered in front, and sent by it in a convergent beam through the manometer tube. The focus of the beam was within the tube and near its most distant end. The gas and oxygen were supplied from gasholders specially constructed for these and similar experiments; long and futile experience of gas from the public mains, or compressed in iron bottles, having shown independent gasholders which could be kept at an unalterable pressure to be essential.

The experiments were conducted thus:—A test-tube *t*, plunged in water, held by the glass *g*, contained the liquid whose vapour was to be examined. Through a cork which stopped the test-tube passed a narrow tube of glass, ending in a small orifice near the bottom of the test-tube, and at a considerable depth below the surface of the liquid. To augment this depth, and to economise the liquid, the lower part of the test-tube was drawn out to half the diameter of its upper part. A second narrow tube passed also air-tight through the cork, and ended immediately beneath it. Both tubes were bent at a right angle above the cork. The manometric tube being exhausted, air freed from its carbonic acid, its moisture, and its suspended matter, was allowed to bubble through the liquid in the test-tube, and to pass thence into the manometric tube. To spare the oxygen in the gasholder it was cut off during the interval between two consecutive experiments, the coal gas being kept continually alight. When the manometric tube was filled, which was always accomplished through an orifice of fixed dimensions, the oxygen was turned on, and the cylinder was allowed to remain for one minute under the action of the intensified flame. During this time a double silver screen S intercepted the radiation. At the end of a minute this screen, which moved on a hinge, was withdrawn, the beam then passing through the mixed air and vapour. The liquid standing in the adjacent leg of the narrow U-tube was immediately depressed, that in the opposite leg being equally elevated. The rise of this latter column above its starting point, marked zero on a millimeter scale, was accurately measured. Double this rise gave the difference of level in the two legs of the U, and this "water pressure" expressed the augmentation of elastic force by the absorption of radiant heat.

Here follow a certain number of the measurements which have been thus made. They do not comprise the whole of the substances examined.

TABLE V.

VAPOURS.

INCREASE of elastic force by Radiant Heat.

Name of Liquid.	Boiling point.	Mean Water pressure.	Character of sound.
1. Sulphuric Ether	35°	300 millims.	Very strong
2. Hydride of Amyl	30°	279 „	„
3. Acetone	58°	267 „	„
4. Bromide of Ethyl	39°	264 „	„
5. Formic Ether	55°	261 „	„
6. Acetic Ether.	74°	248 „	„
7. Acetal.	104°	237 „	„
8. Chloride of Allyl	46°	235 „	„
9. Iodide of Methyl	45°	233 „	„
10. Dichloride of Ethidene . . .	57°	217 „	Strong
11. Nitrate of Ethyl	86°	208 „	„
12. Nitrite of Amyl	99°	205 „	„
13. Chloride of Butyl	69°	185 „	„
14. Butyric Ether	121°	183 „	„
15. Formic Acid	99°	180 „	„
16. Valeral	100°	172 „	„
17. Valerianic Ether	144°	168 „	„
18. Acetate of Propyl	102°	166 „	„
19. Methylic Alcohol	—	162 „	„
20. Iodide of Ethyl.	72°	148 „	Moderate
21. Bromide of Butyl	92°	134 „	„
22. Dutch Liquid	85°	127 „	„
23. Acetate of Butyl	114°	120 „	„
24. Benzol.	81°	117 „	„
25. Carbonic Ether.	126°	108 „	„
26. Chloride of Amyl	102°	105 „	„
27. Chloropicrin.	112°	94 „	„
28. Iodide of Allyl	101°	92 „	„
29. Chloroform	61°	89 „	„
30. Iodide of Butyl.	121°	88 „	„
31. Allylic Alcohol	97°	84 „	„
32. Bisulphide of Carbon	43°	81 „	„
33. Bromide of Amyl	119°	78 „	„
34. Cyanide of Ethyl	98°	77 „	„
35. Butyl Alcohol	110°	72 „	Weak
36. Nitrate of Amyl	147°	67 „	„
37. Oxalate of Ethyl	—	66 „	„
38. Cyanide of Methyl.	82°	64 „	„
39. Tetrachloride of Carbon . . .	77°	58 „	„
40. Bromoform	150°	44 „	„
41. Xylol	140°	44 „	„
42. Amylic Alcohol.	130°	42 „	„

Table V (continued).

Name of Liquid.	Vapours Boiling point.	Mean Water pressure.	Character of sound.
43. Iodide of Amyl	146°	42 millims.	Weak
44. Terebine	160°	39 ,,	,,
45. Cymole	175°	38 ,,	,,
46. Butyric Acid	163°	Very small absorptions and very feeble sounds at ordinary temperatures. Sounds in most cases very strong when liquid is heated to its boiling point.	
47. Butyrate of Amyl	176°		
48. Caprylic Alcohol	180°		
49. Valerianic Acid.	175°		
50. Pure Aniline	184°		
51. Œnanthic Ether	188°		
52. Valerianate of Amyl	196°		
53. Salicylous Acid.	196°		
54. Caproic Acid	205°		
55. Nitro Benzol.	205°		
56. Kreosote	210°		
57. Menthol	213°		
58. Chinoline.	238°		
59. Eugenol	247°		
60. Nicotine	250°		
61. Monobromnapthaline.	277°		
62. Sebasic Ether	308°		

Table VI.

Gases.

Name of gas.	Water pressure.
Chloride of Methyl	350 millims.
Aldehyde	325 ,,
Olefiant Gas	315 ,,
Sulphuric Ether	300 ,,
Nitrous Oxide	198 ,,
Marsh Gas.	164 ,,
Carbonic Acid	144 ,,
Carbonic Oxide	116 ,,
Oxygen	5 ,,
Hydrogen	5 ,,
Nitrogen	5 ,,
Dry air.	5 ,,
Humid air at 50° C.	130 ,,

Sulphuric ether is here inserted with the view of connecting this Table with the last. Of all gaseous bodies hitherto examined chloride of methyl is the most energetic absorber and the most powerful sound producer. After it comes aldehyde, with a boiling point of 21° C. The figure 5 attached to the elementary gases, and to dry air, expresses, not absorption of radiant heat, but expansion, due to contact with the slightly warmed apparatus. The nitrous oxide employed was derived from an iron bottle in which it was preserved for medical purposes. In some of my experiments marsh gas showed itself a better absorber than nitrous oxide. This, for instance, was the case in experiments made in the spring of 1880 with the manometer. The sample of marsh gas wherewith the foregoing result was obtained was very carefully prepared in our chemical laboratory.

The temperature of 50° C. in the case of humid air was obtained in a wooden shed erected in our laboratory. The shed is traversed by two tubes of sheet iron 4 inches in diameter, which carry the heated air and products of combustion from two large ring-burners. It is 8′ 6″ long, 4′ 3″ wide, and 7′ feet high. The temperature of the air within it can be readily raised to 60° C. In the experiment recorded in Table VI. the air was taken from the outside laboratory through a tube passing through the wooden wall of the shed. It was caused to bubble through water contained in a large flask which had been permitted to remain for some time in the warm shed. The mixed air and vapour entered the manometer tube at a temperature some degrees lower than that of the tube itself. Closely examined, all parts of this tube were bright and dry when the vapour-laden air was within it. On permitting the beam from the lime light (produced by coal-gas and oxygen) to pass through the mixture, a prompt rise of 65 millimeters was the consequence. Cutting the beam off the column rapidly returned to zero. The double of 65, or 130 millimeters, gives the difference of level in the two legs of the U-tube.

I have done my best to render these determinations correct. They have been repeated both by myself and my assistant* a great number of times. The first measurements were made in the early part of last year, and were made known in the Royal Institution on the 8th of April, 1881. Difficulties were encountered in obtaining a powerful, and at the same time constant, source of heat. The mixture of coal-gas and oxygen issuing from independent holders was finally resorted to. The sounds are classified into "very strong," "strong," &c., but it is, of course, impossible to say where one class ends and another begins. They shade gradually into each other. But if the middle members of any class be compared with the corresponding members of another class, the difference of sounding power will appear.

§. 10. *Application of Results to Meteorology.*

If it be at length conceded that aqueous vapour exerts upon radiant heat the action which I so long ago ascribed to it, I think the knowledge of this action will prove of importance to the scientific meteorologist. Meteorology, as connected with heat, seems to me to abound in facts which it has hitherto been incompetent to explain. This, for example, I hold to be the case as regards the celebrated observations of PATRICK WILSON of Glasgow, made a century ago. WILSON brought strongly into light the great differences which sometimes exist between the temperature of the earth's surface, and of the air at a small elevation above the surface. His letter to Dr. MASKELYNE on this subject is published in the Philosophical Transactions for 1780 under the title:—"*An account of a most extraordinary degree of cold at Glasgow, together with some new experiments and observations on the comparative temperature of hoar frost and the air near it.*" On the afternoon of the 13th of January, 1780, the cold was intense, a thermometer at the high window of the observatory pointing, at 7 P.M., to 0° FAHR. At 8 P.M. WILSON and Dr. IRVINE laid two thermometers upon the snow, and hung up two others in the air 2 feet above the snow. Here follow the temperatures observed on the evening of the 13th and on the morning of the 14th of January:—

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* Who has aided me in this investigation with his usual zeal and intelligence.

Time of observation.	Snow temperature.	Air temperature.
8½ P.M.	−12°	−0°
9 "	−14°	−2°
10 "	−14°	−4°
11 "	−17°	−6°
11½ "	−18°	−6°
½ A.M.	−20°	−8°
1 "	−23°	−7°

The sign — signifies that the temperatures were all below zero FAHRENHEIT. These temperatures amply justifiy WILSON's statement that the cold was "extraordinary." Coexistent moreover with the general cold we have a difference of 16° between the temperature of the surface and that of the air 2 feet above it. Had the air thermometer been hung 10 feet, instead of 2 feet, above the surface the difference would have been still greater. The thermometer, moreover, must have been chilled, not only by its immersion in cold air, but also by its own radiation against the intensely cold snow. The chilling of the superficial snow was purely an effect of radiation. Beneath the surface its temperature reached +14°. WILSON filled a bread-basket with this warm snow at 2½ A.M. on the 14th. Within half an hour it had fallen 24°, and in two hours 32°.

I venture to predict that if WILSON's experiment be repeated during the cold of a Canadian winter the same result will be obtained; and it seems to me that until the action of water vapour upon radiant heat had been discovered no explanation of the phenomenon could have been given. It was accepted but not accounted for. On the night of WILSON's observations "a light air was felt coming from the east." With such an "air" and such a temperature the quantity of water vapour in the atmosphere must have been infinitesimal. Dry air being a practical vacuum to the rays of heat, were the vapour-screen entirely removed, the earth would find itself exchanging temperatures with celestial space, and the superficial chill would be commensurate. In WILSON's case, though the vapour was not abolished, it was so far diminished as to produce the observed refrigeration. Meteorologists, I am informed, sometimes say that laboratory experiments, however well performed, have but little application to their field of observation.* I, on the other hand, submit that such experiments are necessary to rescue their science from empiricism. What could WELLS have done with dew had he not been preceded by LESLIE and RUMFORD? His whole theory is an application of results obtained in the laboratory.†

What I have stated regarding WILSON applies also to SIX, who concluded from his experiments "that the greatest differences at night in point of temperature, between bodies on the surface of the earth and the atmosphere near it, are those which take place in very cold weather." This is quoted from WELLS,‡ who, in his Essay on Dew, recurs more than once to the subject. He signalises, but does not explain, "the greater difference which takes place in very cold weather, if it be calm and clear, between the temperatures of the air and of bodies on the earth at night, than in equally calm and clear weather in summer."§ A considerable number of observations bearing upon this point are scattered through the Essay. The radiant power of the air being practically *nil*, it retains for a considerable time the warmth imparted to it during the day, while when it is dry, the rays from the surface of the earth pass unimpeded through it. Hence the relative refrigeration of the surface.‖

In regard to the action of water vapour MAGNUS considered experiment superfluous, as the phenomenon of dew sufficed to prove me wrong. If the vapour possessed the power which I ascribed to it, he contended that dew could not be formed. It is not difficult to dispose of this objection. The formation of dew and superficial refrigeration are connected, not by coincidence but by opposition. I would venture to predict that where the one is great the other, in general, will be small. "Very little dew," says

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* Mr. HILL, the Meteorological Reporter for the North-Western Provinces of India, writes thus:—"There is even, on the part of some, an evident reluctance to accept the decision of laboratory experiments on the question of atmospheric absorption as final, however ingenious, varied, and consistent with one another the experiments may be."—Proc. Roy. Soc., vol. 33, p. 216.

† "Its complete theory," says WELLS, "could not possibly, in my opinion, have been attained, before the discoveries on heat were made, which are contained in the works of Mr. LESLIE and Count RUMFORD."—Essays, p. 191.

‡ Essays, p. 176. WELLS thus generously refers to the labours of WILSON. "Indeed, several of my experiments upon dew were only imitations of some which had been previously made on hoar frost, by that ingenious and worthy man."—Essays, p. 151.

§ Ibid., p. 188.

‖ It ought to be stated that, contrary to SIX and to WELLS, Mr. GLAISHER has found that "the differences between the temperature of the air and of bodies on the earth at night, in equally clear and calm weather, were the same at every period of the year." (Phil. Trans., 1847, p. 126.) He moreover records differences considerably in excess of those observed by WILSON and by WELLS. Keeping the action of aqueous vapour in view, the elaborate paper of Mr. GLAISHER might repay further discussion.

WELLS, "appeared on the two nights of the greatest cold I have ever observed on the surface of the earth, relatively to the temperature of the air, both of them having occurred after a long tract of dry weather."* This evidence is specially valuable in view of the fact that WELLS knew nothing of the action of water vapour on radiant heat. On another occasion he observed a difference of 9½ degrees between the temperature of the air and that of wool placed upon the earth, without any deposition of dew whatever upon the chilled wool.† He supplements these observations by one equally important in the opposite direction. "On the night," he writes, "which afforded the most copious dew ever observed by me, the cold possessed by the grass beyond that of the air was for the most part only 3° and 4°."‡ The smallness of the refrigeration in this instance, and the copiousness of the dew, I refer to one and the same cause; namely, the abundance of vapour. Heavy dew implies this abundance; abundant vapour, if not too local, implies checked radiation, and checked radiation tends to abolish the difference of temperature between air and soil.

WELLS had a theory of his own to account for the association of moderate refrigeration and heavy dew. The heat rendered free by the condensation of the vapour to liquid "prevented" the cold. He tried to determine the effect of condensation by the following experiment.§ To 10 grains of wool he added 21 grains of water, this being the quantity of dew deposited on wool in one of his observations. He placed the moistened wool in a saucer on a feather bed in a room, and determined the chill produced by its evaporation. After eight hours, while the wool still retained 2½ grains of moisture, its temperature was 4° lower than that of a dry saucer placed near it on the same feather bed. When the process is reversed, condensation instead of evaporation coming into play, the foregoing amount of heat, WELLS contended, would be liberated on the grass, and thus prevent inordinate refrigeration.

In thus reasoning WELLS went to the limit of the knowledge of his time, and the explanation here given is a philosophical one. But I do not think it a sufficient explanation. The grass is exposed to the open atmosphere, and the heat developed by every successive film of moisture condensed upon its blades, is instantly wasted by radiation. Those who are accustomed to work with the thermopile know how rapidly the associated galvanometer needle falls from a high deflection to zero, when the heat incident upon the pile is suddenly cut off. A similar rapidity of waste would assuredly occur during the slow formation of dew. The heat of condensation could not for this reason be housed in the manner supposed by Wells.[*] The true explanation I hold to be that already indicated—the checking of radiation by vapour, the abundance of which was indicated by the copious deposition of dew.

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* Essays, p. 186.

† Ibid., p. 183. WELLS sometimes found wetted wool to lose weight, while dry wool gained no weight though lowered many degrees below the temperature of the air, p. 184.

‡ Ibid., p. 169. From a remark occurring at page 135, it may be inferred that the night here referred to was that common to the 29th and 30th of July, 1813. On the two occasions first mentioned, when there was but little dew, the grass was in one instance 12°, and in the other 14° colder than the air.

§ Essays, p. 187.

If the experiments of Wilson could be made in an atmosphere still colder than that in which he worked,—on a large plain, for instance, and in a country remarkable for the dryness of its air—Wells considered that a difference of at least 30° would be observed on serene nights between the air and a downy substance placed on the earth. And as Six had found the air temperature at an elevation of 220 feet to be 10° higher than at 7 feet, these 10° being added to the 30° would make the surface at least 40° colder than the air at the height of 200 or 300 feet. With all this I agree. I would go even further, and reiterate here a statement made by me nineteen years ago, that the withdrawal of the aqueous vapour of our atmosphere, for a single calm night, would kill every plant in England capable of being killed by a freezing temperature.

Pictet, I believe, was the first to notice that the temperature of the air near the earth's surface on serene nights diminished as the surface was approached, the sequence of the day temperatures being thus inverted. To account for the chilling of the air say at 10 feet above the earth's surface beyond that at 100 feet above the surface, Wells invoked the radiant power of the air itself. It is chilled, he thought, by its own emission against the cold earth underneath. Wells takes great pains to prove that the air possesses this power; and if not the air, the floating matter of the air will, he contends, exert the necessary radiation. Difficulties of this nature not unfrequently crop up in works on meteorology, but they disappear in presence of the fact, that mixed with the air is a gaseous constituent, small in quantity, but capable of producing the effects needing explanation.

As an example of such difficulties, I have already referred to Sir John Leslie's paper "On certain impressions of cold transmitted from the Higher Atmosphere."[†]

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[*] Wells himself observed in grass a fall of temperature of 7° in twenty minutes. This gives us some notion of the rapidity with which a radiant so powerful as water would dispose of its heat.—Essays, p. 157.

At the instance of my friend Mr. Francis Galton, and with the kind sanction of the Meteorological Council, the following instructive observations, showing the temperatures recorded by two thermometers, the one placed on cotton-wool resting on the earth, and the other hung at a height of four feet in the air, were recently made by Mr. Whipple at Kew:—

Time.	Air.	Wool.
4.20 P.M.	34·8°	33·2°
„ 25 „	32·5	27·6
„ 30 „	32·4	25·7
„ 35 „	32·4	23·4
„ 40 „	32·2	21·7
„ 45 „	32·2	20·7

The rapidity of radiation is well shown by these observations, an exposure of twenty-five minutes sufficing to establish a difference of 11·5°.

[†] Transactions Roy. Soc. of Edinburgh, vol. viii., p. 483.

powerful refrigeration. I close these references to mist and haze by mentioning a most striking observation made by WELLS on the 1st January, 1814. "I found," he says, "during a dense fog, while the weather was very calm, a thermometer lying on grass thickly covered with hoar frost, 9° lower than another suspended in the air 4 feet above the former."* Here, as before, low temperature implies scanty vapour, the absence of which enabled the grass to pour its heat even through the interstices of a dense fog.†

I could draw still further on this admirable Essay in illustration of the thesis which I have so long defended. As a repertory of valuable facts and penetrative arguments it probably stands unrivalled in the literature of meteorology. One point remains which cannot be passed over. It has reference to the part played by clouds in arresting and returning the radiation from the earth. "No direct experiments," says WELLS, "can be made to ascertain the manner in which clouds prevent, or occasion to be small, the appearance of a cold at night, upon the surface of the earth, greater than that of the atmosphere; but it may, I think, be firmly (fairly ?) concluded from what has been said in the preceding article, that they produce this effect, almost entirely, by radiating heat to the earth, in return for that which they intercept in its progress from the earth towards the heavens."‡ WELLS had the strongest analogies to adduce in favour of this view. He placed boards and sheets of paper above his thermometers, thus screening them from the clear sky; and in that beautiful passage where he speaks of "the pride of self knowledge," and refers to the simple devices which experience had taught gardeners to apply for the safety of their plants, he mentions the protection which even a thin cambric handkerchief can afford to thermometers over which it is spread. He was irresistibly led to conclude that clouds acted in the same fashion, and that when they occupied the firmament, they sent back to the earth the heat incident upon them, exactly as the board, and the paper, and the cambric, sent it back in experiments made close to the surface of the earth.

But in the enunciation of this hypothesis his knowledge and penetration as an observer came, as usual, into play. He is careful to distinguish between high clouds and low clouds. "Dense clouds," he says, "near the earth," must possess the heat of the lower atmosphere, and will therefore send to the earth as much, or nearly as much, heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky, yet being from this elevated situation colder than the earth, will radiate to it less heat than they receive from it, and may consequently admit of bodies on its surface becoming several degrees colder than the air."§

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* Ibid., p. 158.

† Mr. GLAISHER moreover has found differences of from 10° to 12° between grass and air "at times when the sky has been free from clouds but not bright, haze and vapour being prevalent." (Phil. Trans., 1847, p. 145.)

‡ Essays, p. 205.

§ Ibid., p. 206. "If," says WELLS, in another place, "the clouds were high and the weather calm, I

Magnus urged this point against me, and I may be permitted to say that I always considered it one of his strongest points; my holding of this opinion being however dependent on the views which I entertained, and which were opposed to those of Magnus, regarding the relation of liquid to vapour. If, as I believe, the absorbent power is not enhanced by condensation—if in this respect water behaves like hydride of amyl and sulphuric ether—then I do not think that such a process of reverberation, between earth and clouds, as that assumed by Wells is possible. The aqueous vapour in a very few thousand feet of air, of average humidity, would, if condensed, form a layer of water 0·5 of an inch in thickness, and through such a layer, or even through a thinner layer, the earth's radiation could not pass. If the earth's radiation reach the clouds it must be by a process similar to that of handing buckets from man to man in the case of a fire. The heat must be taken up and re-radiated, we know not how many times, before the clouds are reached. I do not, however, think this mechanism of discharge necessary. Low clouds will not form above exposed thermometers, in weather previously serene, unless some change has occurred in the atmosphere; and change may occur where no cloud reveals it. It may extend, and in most cases probably does extend, from the low clouds to the earth. I think it in the highest degree probable that in most, if not in all the cases cited by Wells, of thermometers rising when clouds were formed overhead, the precipitation was due to the intrusion of humid air, the humidity extending invisibly from the clouds downwards. To this, I believe, rather than to any immediate exchange of temperature with the clouds, the rapid and considerable changes of temperature referred to by him at pp. 156 and 157 of the Essay are to be ascribed. Future observations will, doubtless, bring this view to an experimental test.

I here recur with renewed pleasure, to a paper published by General Strachey in the Philosophical Magazine for July, 1866. It was probably intended as a reply to the strictures of Magnus; and to me it appears cogent in the highest degree. General Strachey calculated the fall of temperature from $6^h\ 40^m$ P.M., Madras time, to $5^h\ 40^m$ next morning, for a certain number of days, selected as sufficiently clear. He also calculated the mean vapour tension during the nights, and tabulated the results according to the quantity of vapour for the years 1841, 1842, 1843 and 1844. In such observations, as pointed out by Strachey, discrepancies are to be expected, but the general result is unmistakable, that the fall of temperature by radiation is greatest when the air is driest, and least when the air is most humid. A series of observations made at Madras between the 4th and the 25th of March, 1850, are particularly suitable for the illustration of this law of action. During the period referred to "the sky remained remarkably clear, while great variations of the quantity of vapour took place." Here are the results as tabulated by General Strachey:—

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have sometimes seen on grass, though the sky was entirely hidden, no very inconsiderable quantity of dew."—Ibid., p. 128.

powerful refrigeration. I close these references to mist and haze
most striking observation made by WELLS on the 1st January,
says, "during a dense fog, while the weather was very calm
grass thickly covered with hoar frost, 9° lower than an
4 feet above the former."* Here, as before, low temp
the absence of which enabled the grass to pour its he
a dense fog.†

	·554.	·435.
1	13°·1	16°·5

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RACHEY as to the action of aqueous
the results :—

·4.	·737.	·670.	·576.	·511.	·394.
12°·4	15°·1	19°·3	22°·2	24°·3	27°·0

is the exact complement of the last. *There* the *fall* of temperature was promoted by the withdrawal of the vapour. *Here* the *rise* of temperature essentially promoted by the same cause.*

But the most impressive illustration of the action of aqueous vapour is now to be referred to. In 1865 I subjected to examination the radiation from the electric light produced by a battery of 50 of GROVE's cells, and found, by prismatic analysis, the invisible calorific radiation to be 7·7 times the visible. The determination was afterwards made by the method of filtration, whereby the one class of rays was detached with great sharpness from the other, and both of them rendered measurable. By this method the invisible radiation was found to be 8 times the visible. A close agreement was therefore established between the results of the two methods. Computed from the diagram of MÜLLER the invisible radiation of the sun is twice the visible. This smaller ratio might, of course, be referred to the original quality of the solar

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[*] Mr. HILL, Meteorological Reporter for the North-West Provinces of India, in a paper recently presented to the Royal Society describes an attempt to determine the "Constituent of the Atmosphere which absorbs Radiant Heat." He uses for this purpose the careful observations made by Messrs. J. B. N. HENNESSEY and W. H. COLE, at Mussoree and Dehra respectively. From the absence of symmetry in the quantities of heat received by the actinometer on both sides of noon at Mussoree, and from the existence of this symmetry at Dehra, he infers the periodic lifting and lowering of the absorbing constituent above and below the higher station. He finds the variation of the absorption coefficient to follow the variations of vapour tension. From this and from a similar result obtained by a second method of calculation, he draws the conclusion "that there can be very little error in agreeing with Dr. TYNDALL that the absorptive power of dry air is sensibly nothing, and that the total absorptive power of the atmosphere is due to the water vapour it contains." A most interesting discourse on Solar Heat, by M. VIOLLE, of Grenoble, will be found in the Revue Scientifique for 1878, p. 944. I guard myself against saying that the diathermancy of dry air is *perfect*.

Tension of vapour.	·888.	·849.	·805.	·749.	·708.	·659.	·616.	·554.	·435.
Fall of temperature from $6^{h}\ 40^{m}$ P.M. to $5^{h}\ 40^{m}$ A.M.	6·0	7·1	8·3	8·5	10·3	12·0	12·1	13·1	16·4

These results, if correct, and I am not aware that they have ever been questioned, show in the most impressive manner the influence of the aqueous vapour of our atmosphere on our planet's radiation. As the vapour diminishes, the door opens, which permits the escape of the earth's heat. The halving of the vapour tension nearly trebles the refrigeration of the thermometer.

Equally clear is the evidence given by General STRACHEY as to the action of aqueous vapour upon the radiation of the sun. Here are the results :—

Tension of vapour.	·824.	·737.	·676.	·576.	·521.	·354.
Rise of temperature from $5^{h}\ 40^{m}$ A.M. to $1^{h}\ 40^{m}$ P.M.	12·4	15·1	19·3	22·2	24·3	27·5

This table is the exact complement of the last. *There* the *fall* of temperature was powerfully promoted by the withdrawal of the vapour. *Here* the *rise* of temperature is powerfully promoted by the same cause.*

But the most impressive illustration of the action of aqueous vapour is now to be referred to. In 1865 I subjected to examination the radiation from the electric light produced by a battery of 50 of GROVE'S cells, and found, by prismatic analysis, the invisible calorific radiation to be 7·7 times the visible. The determination was afterwards made by the method of filtration, whereby the one class of rays was detached with great sharpness from the other, and both of them rendered measurable. By this method the invisible radiation was found to be 8 times the visible. A close agreement was therefore established between the results of the two methods. Computed from the diagram of MÜLLER the invisible radiation of the sun is twice the visible. This smaller ratio might, of course, be referred to the original quality of the solar emission, the ratio holding good up to the surface of the sun. But having placed, as I thought beyond doubt, the action of aqueous vapour on radiant-heat, and believing the action of the vapour to be substantially the same as that of water, I reasoned and experimented as follows in 1865:—"The sun's rays, before reaching our earth, have to pass through the atmosphere, where they encounter the atmospheric vapour which exercises a powerful absorption on the invisible calorific rays. From this, apart from other considerations, it would follow that the ratio of the invisible to the visible radiation in the case of the sun must be less than in the case of the electric light. Experiment, we see, justifies this conclusion. If we cause the beam from the electric lamp to pass through a layer of water of suitable thickness, we place its radiation in approximately the same condition as that of the sun; and on decomposing the beam, after it has been thus sifted, we obtain a distribution of heat closely resembling that observed in the solar spectrum."

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* Mr. HILL, Meteorological Reporter for the North-West Provinces of India, in a paper recently presented to the Royal Society describes an attempt to determine the "Constituent of the Atmosphere which absorbs Radiant Heat." He uses for this purpose the careful observations made by Messrs. J. B. N. HENNESSEY and W. H. COLE, at Mussoree and Dehra respectively. From the absence of symmetry in the quantities of heat received by the actinometer on both sides of noon at Mussoree, and from the existence of this symmetry at Dehra, he infers the periodic lifting and lowering of the absorbing constituent above and below the higher station. He finds the variation of the absorption coefficient to follow the variations of vapour tension. From this and from a similar result obtained by a second method of calculation, he draws the conclusion "that there can be very little error in agreeing with Dr. TYNDALL that the absorptive power of dry air is sensibly nothing, and that the total absorptive power of the atmosphere is due to the water vapour it contains." A most interesting discourse on Solar Heat, by M. VIOLLE, of Grenoble, will be found in the Revue Scientifique for 1878, p. 944. I guard myself against saying that the diathermancy of dry air is *perfect*.

If therefore, we could get above the vapour-screen which swathes the earth, the "powerful absorption" referred to in the paragraph just quoted would disappear, the ratio of the invisible to the visible solar rays being augmented correspondingly. That such would be the case I have long taken for granted, but I hardly hoped for a corroboration so impressive as that furnished by the recent observations of Professor LANGLEY, in the Sierra Nevada Mountains of California. Professor LANGLEY is known to have highly distinguished himself by researches on radiant heat, with instruments of his own invention—he writes to me thus from Mount Whitney, California, Sept. 10, 1881:—

"I received your letter just as I was starting on the expedition to this point of which I wrote. I much regretted that I had not time to provide myself with your mercury pyrheliometer, so I have been obliged to use the old form, with its many disadvantages.

"Our route here has led us through the dryest parts of this continent, and across rainless deserts to this mountain, where the air is perhaps drier than at any other equal altitude ever used for scientific investigation. I write from an altitude of 12,000 feet, while the 'Peak' rises nearly 3,000 more above me. I have been successful in bringing up, and using here, the rather complex and delicate apparatus for investigating the absorption of the atmosphere on homogeneous rays, throughout the visible and invisible spectrum.

"You may be interested in knowing that the result indicates a great difference in the *distribution* of the solar energy here from that to which we are accustomed in regions of ordinary humidity; and that while the evidence of the effect of water vapour on the more refrangible rays is feeble, there is, on the other hand, a systematic effect due to its absence which shows by contrast its power on the red and ultra-red in a striking light.

"These experiments also indicate an enormous extension of the ultra-red spectrum beyond the point to which it has been followed below; and being made on a scale different from that of the laboratory—on one indeed as grand as nature can furnish—and by means wholly independent of those usually applied to the research, must, I think, when published, put an end to every doubt as to the accuracy of the statements so long since made by you, as to the absorbent power of this agent over the greater part of the spectrum, and as to its predominant importance in modifying to us the solar energy.

"I am, with much regard,
"Very truly yours,
"S. P. Langley."

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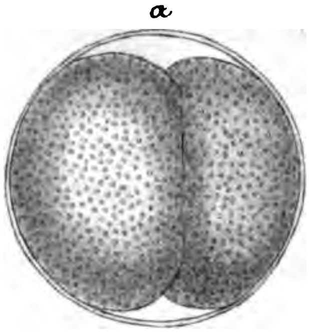

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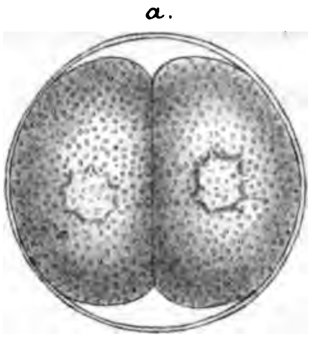

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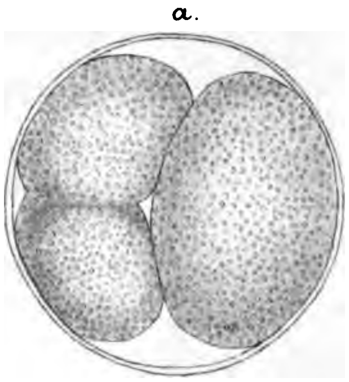

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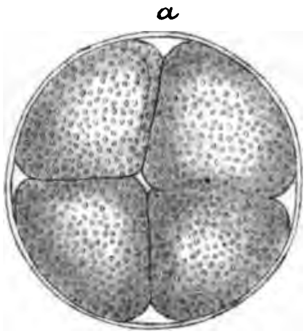

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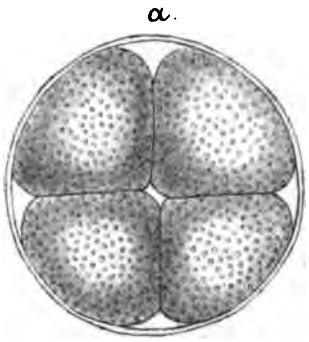

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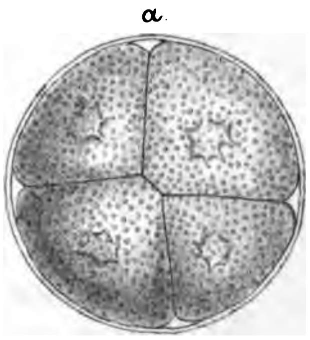

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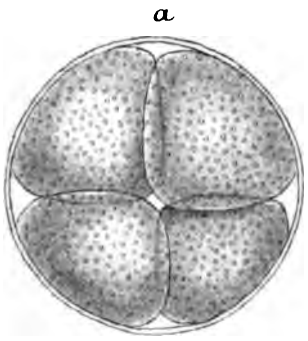

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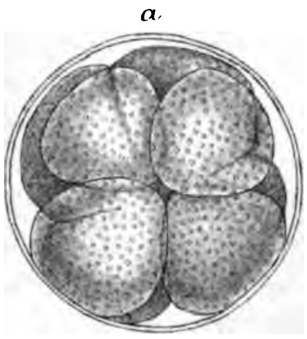

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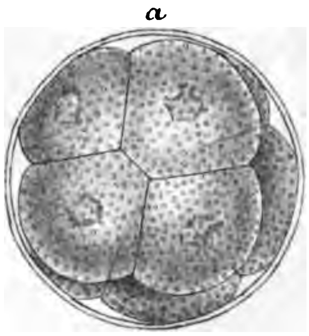

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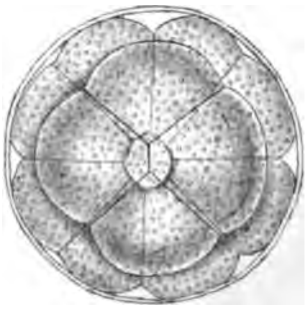

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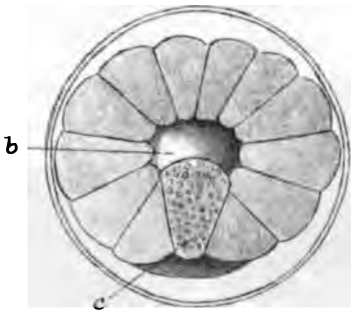

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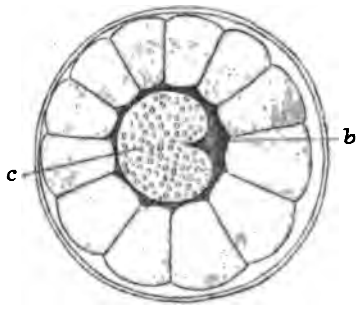

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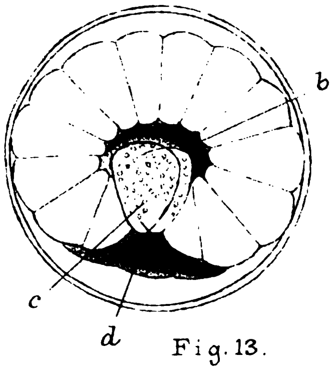

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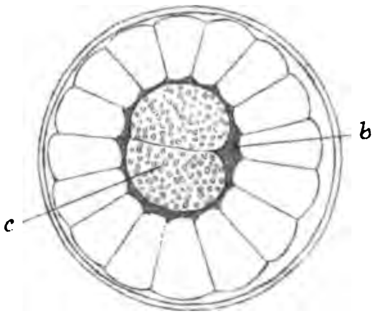

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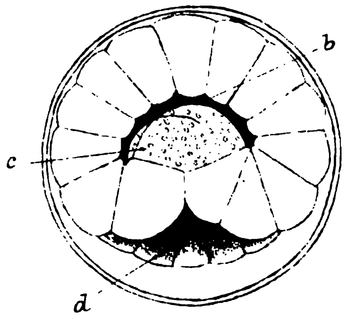

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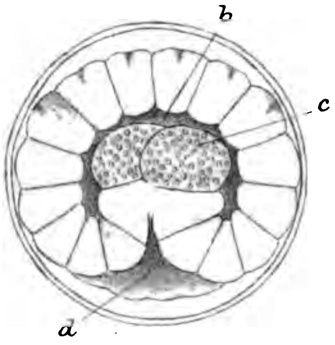
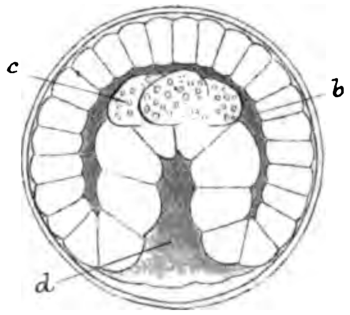


Fig. 16.



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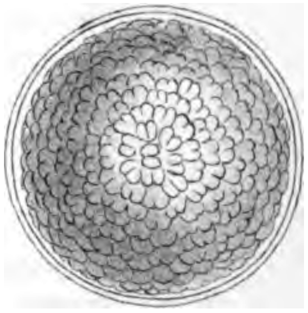

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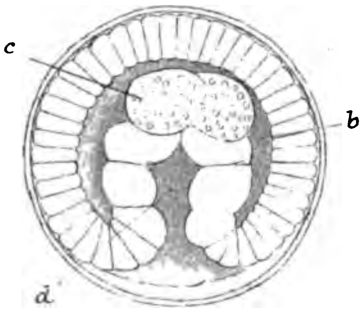

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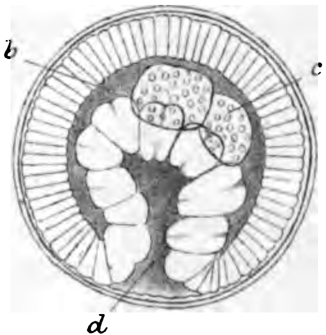

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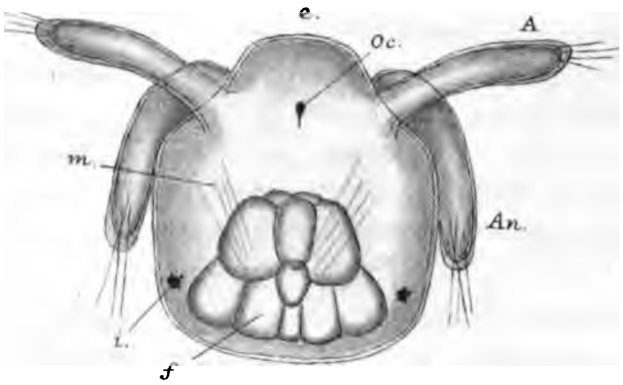

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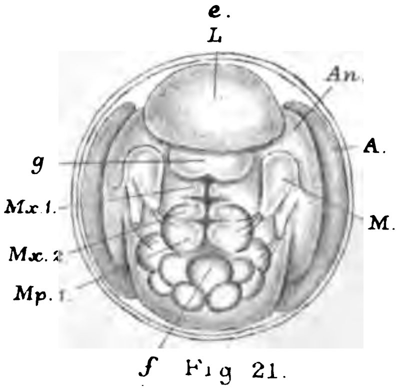

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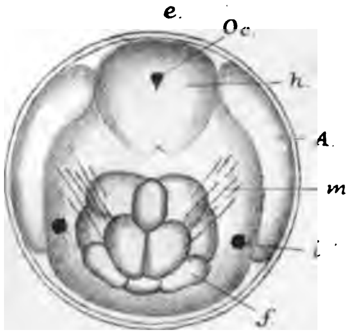


Fig. 22.

W. K. B. ad nat. del.

West Newman & Co lith

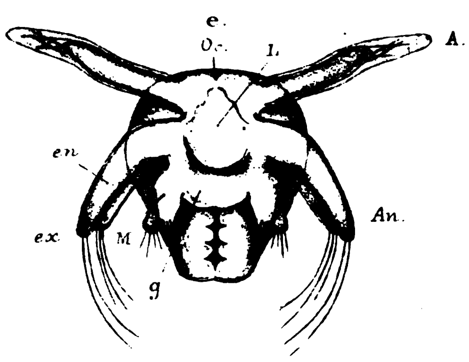
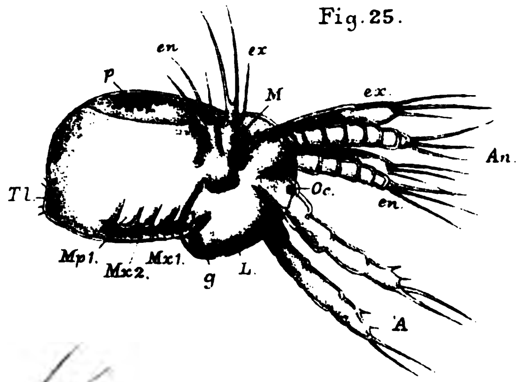
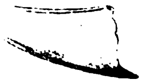


Fig. 24.

Fig. 25.





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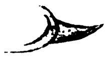
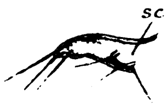


Fig. 29



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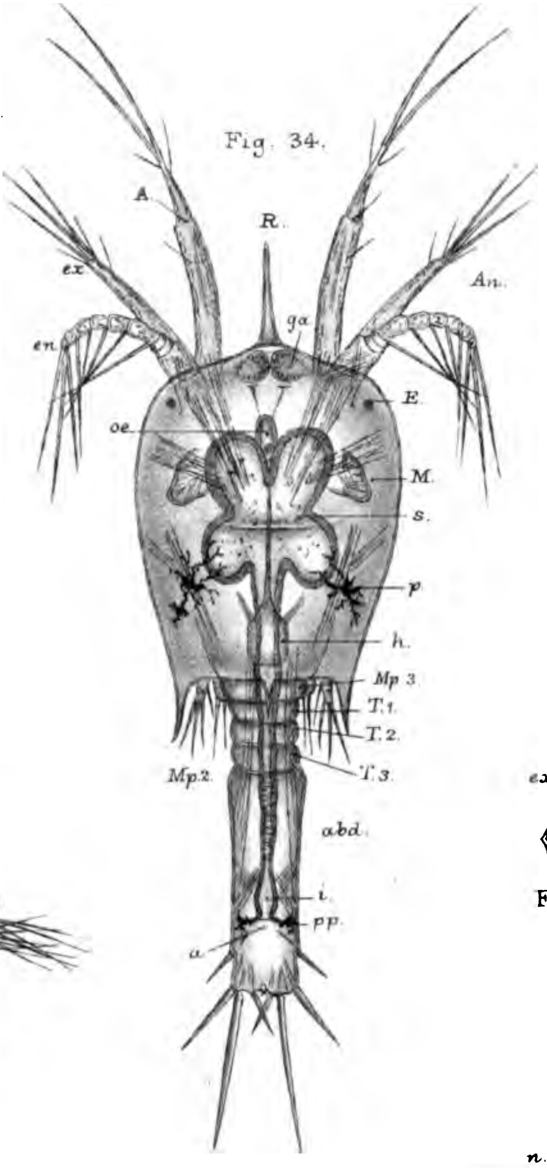

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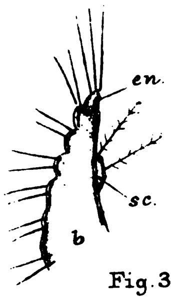

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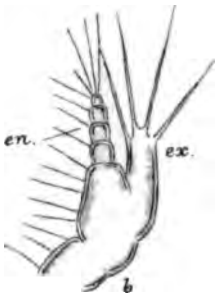

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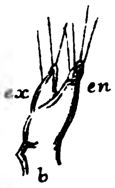


Fig. 33

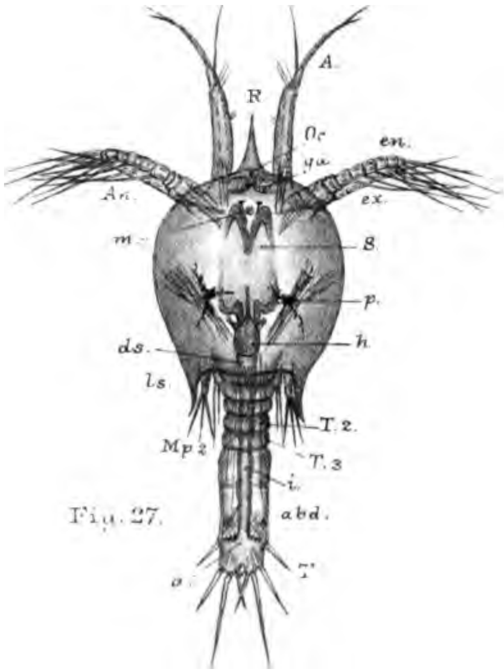
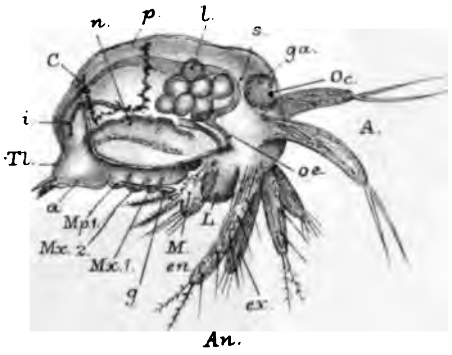


Fig. 27.

Fig. 26.



W. K. B. ad nat. del.

West Newman & Co. imp.

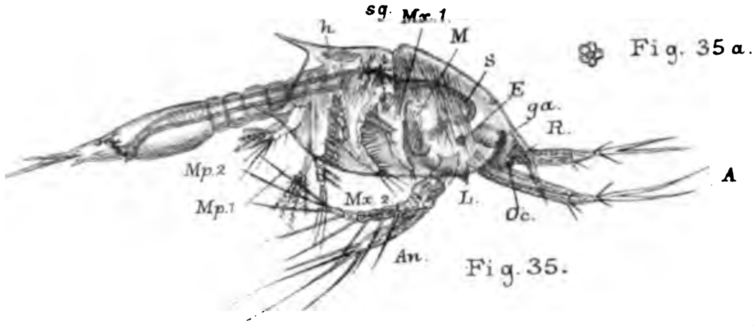
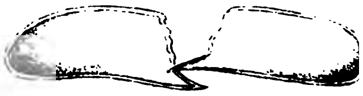


Fig. 35 a.

Fig. 35.



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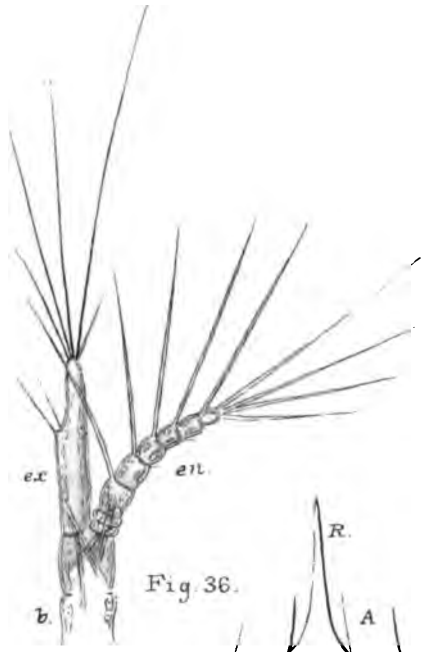

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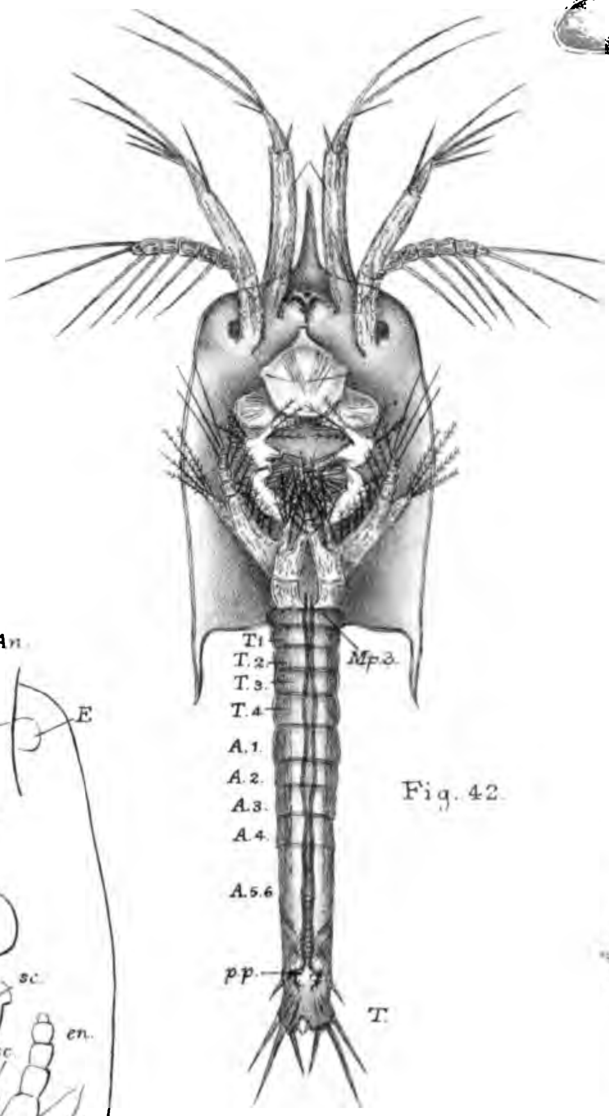

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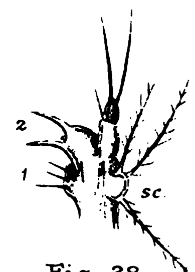

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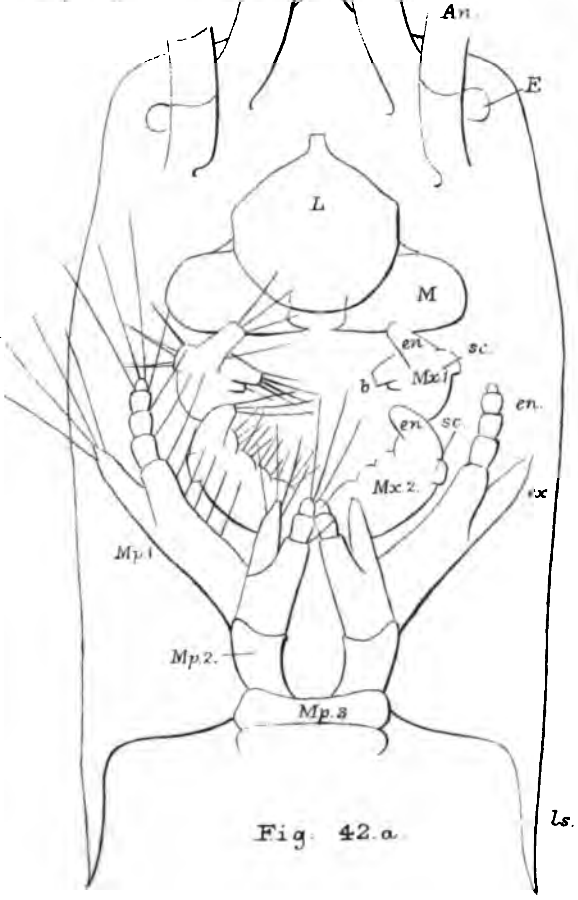

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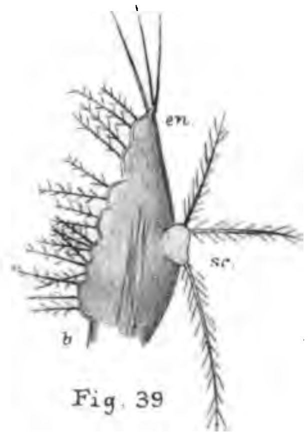

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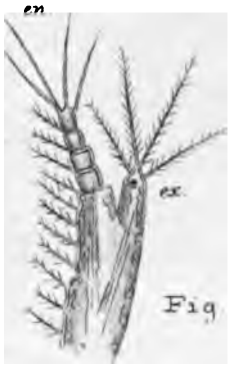

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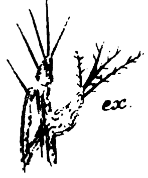

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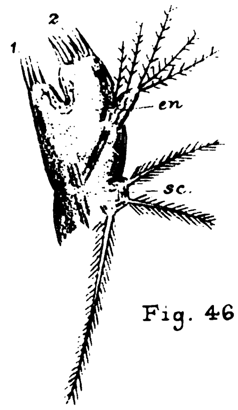

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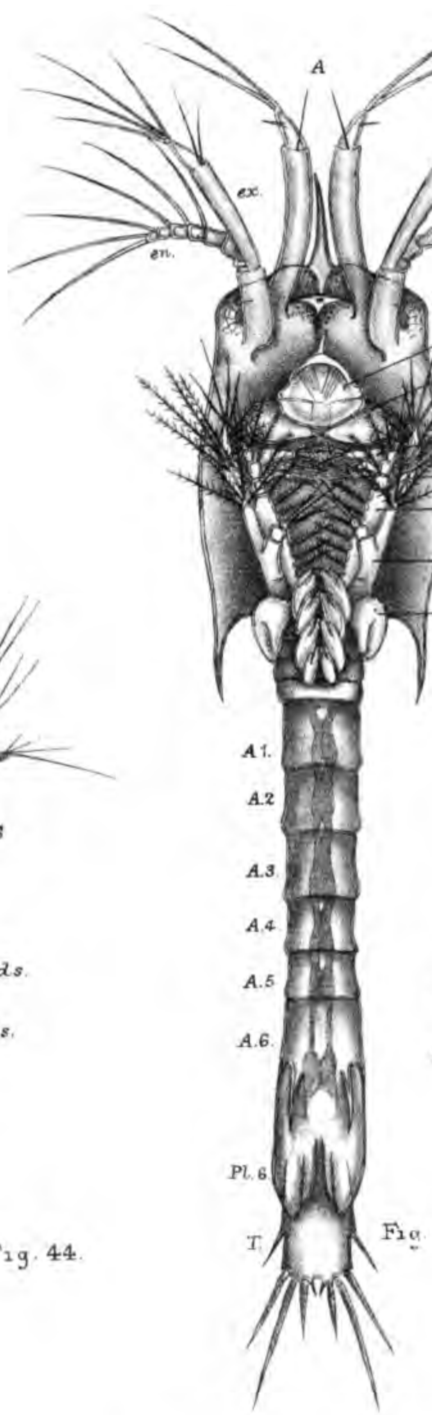

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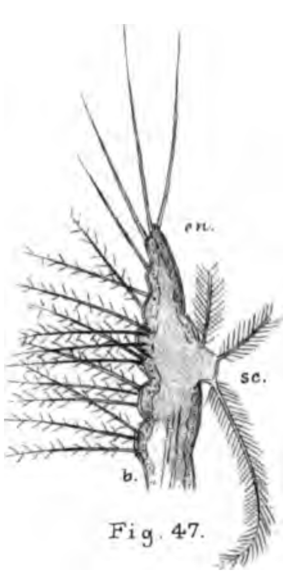

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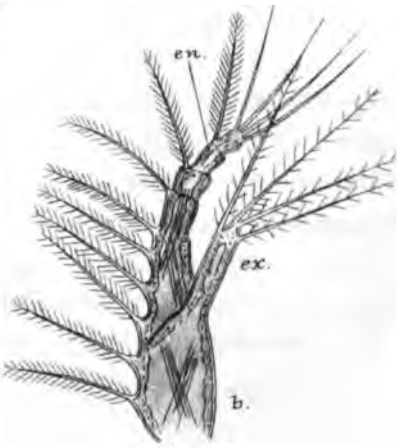

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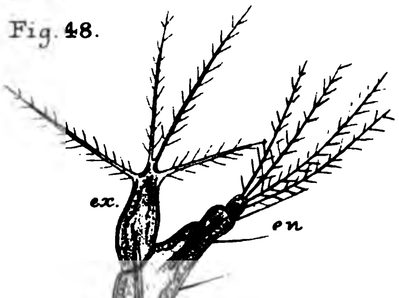

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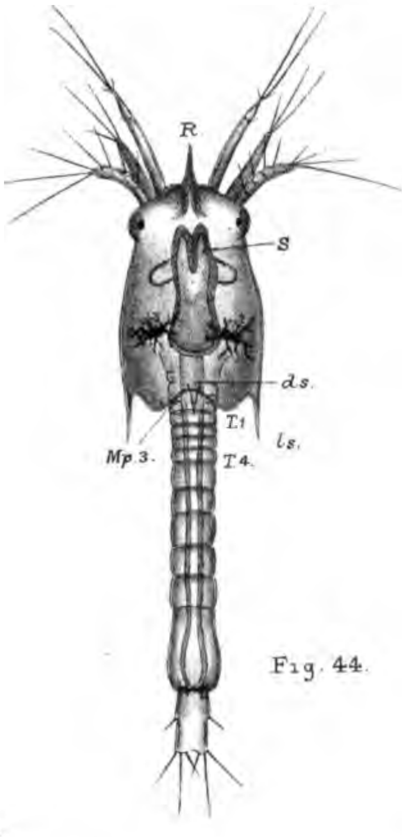

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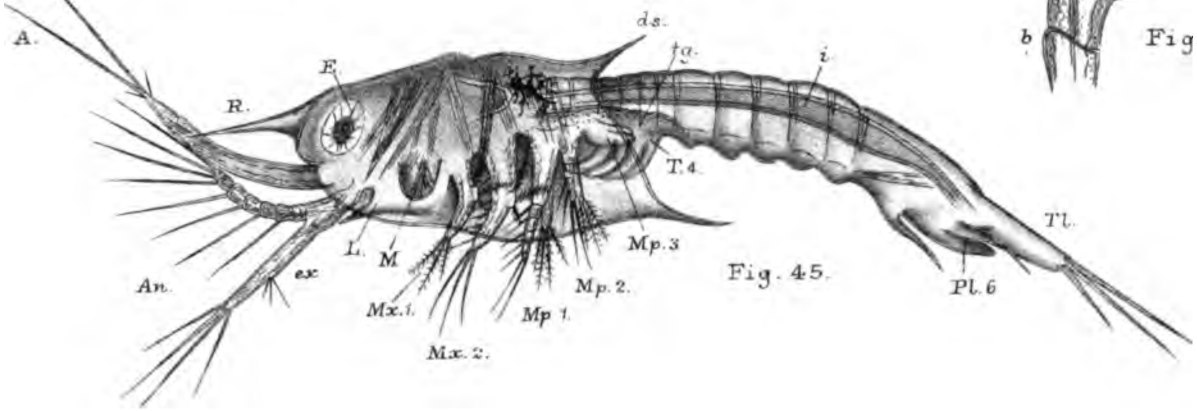


Fig. 45.

W. K. B. ad nat. del.
West Newman & Co. lith.

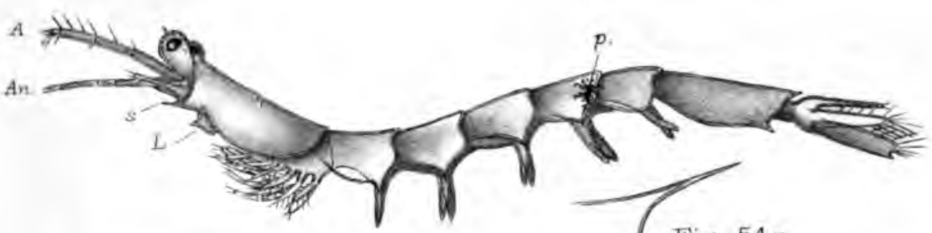
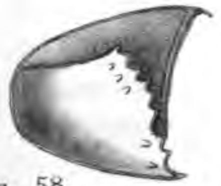


Fig. 54.

Fig. 54 a.



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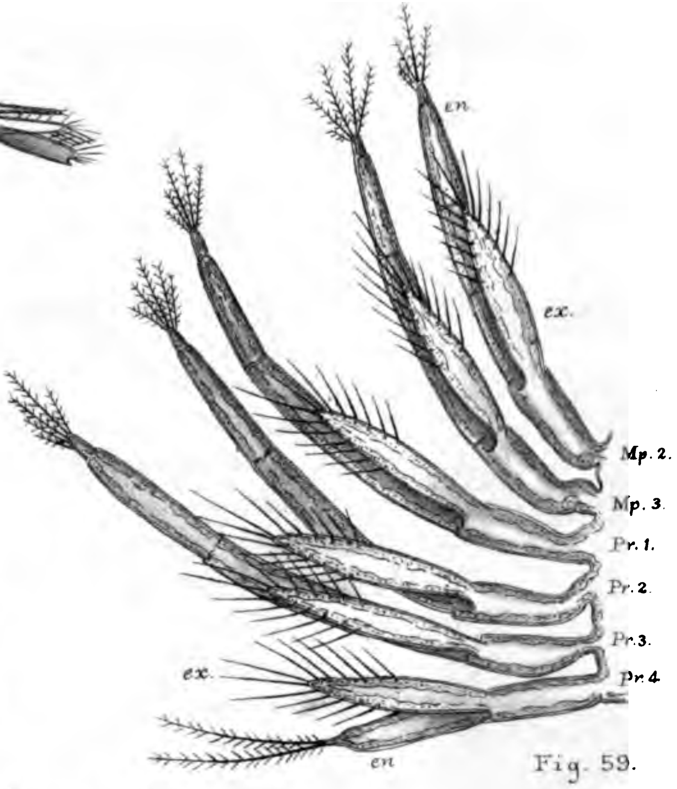

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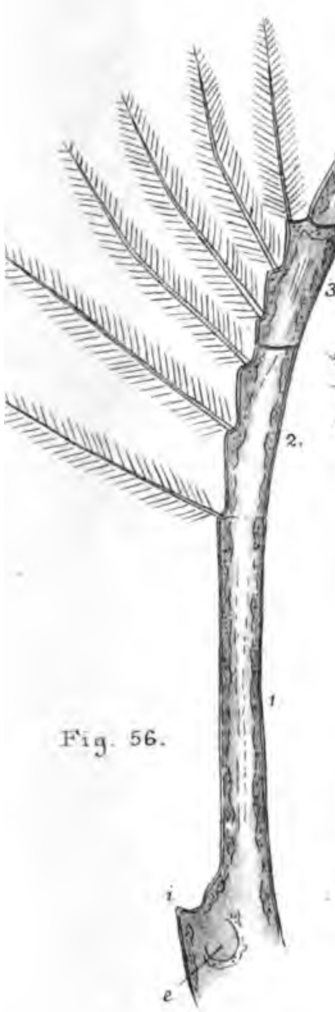

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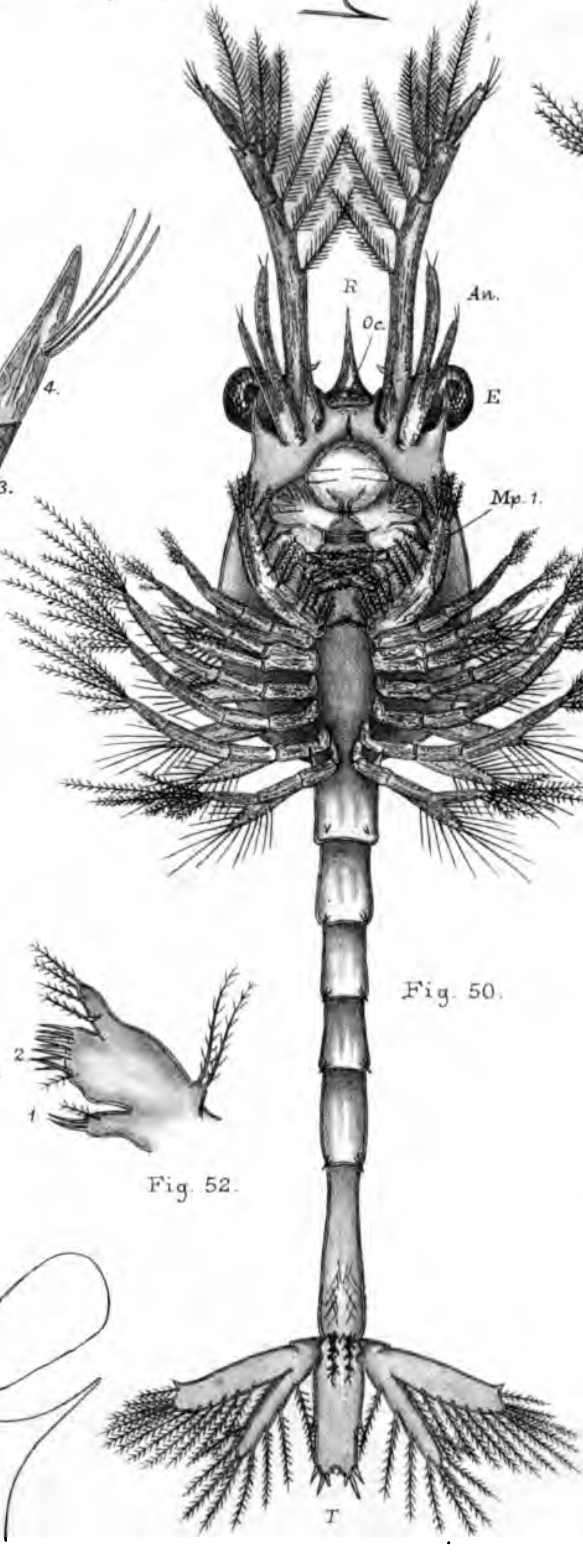

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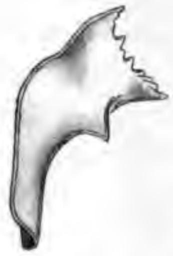

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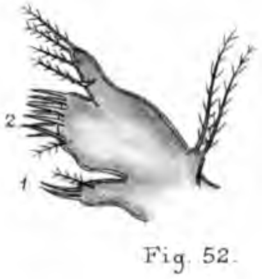

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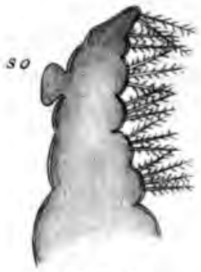

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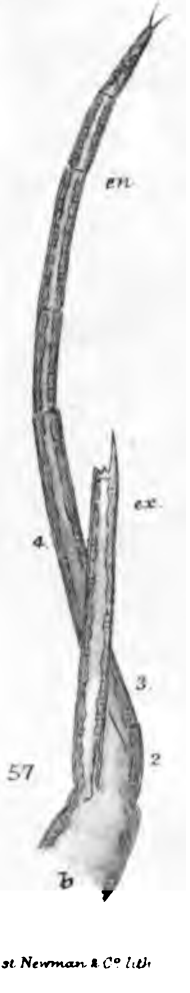


Fig. 57

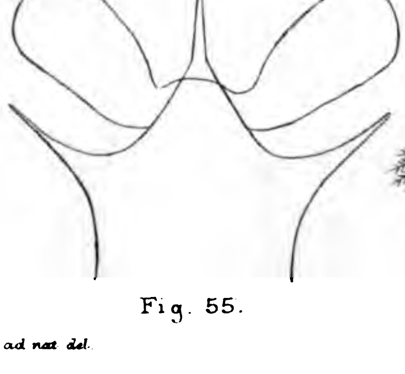


Fig. 55.

K. B. ad nat. del.

West, Newman & Co. lith.

Brooks.

Phil. Trans. 1882. Plate 7.

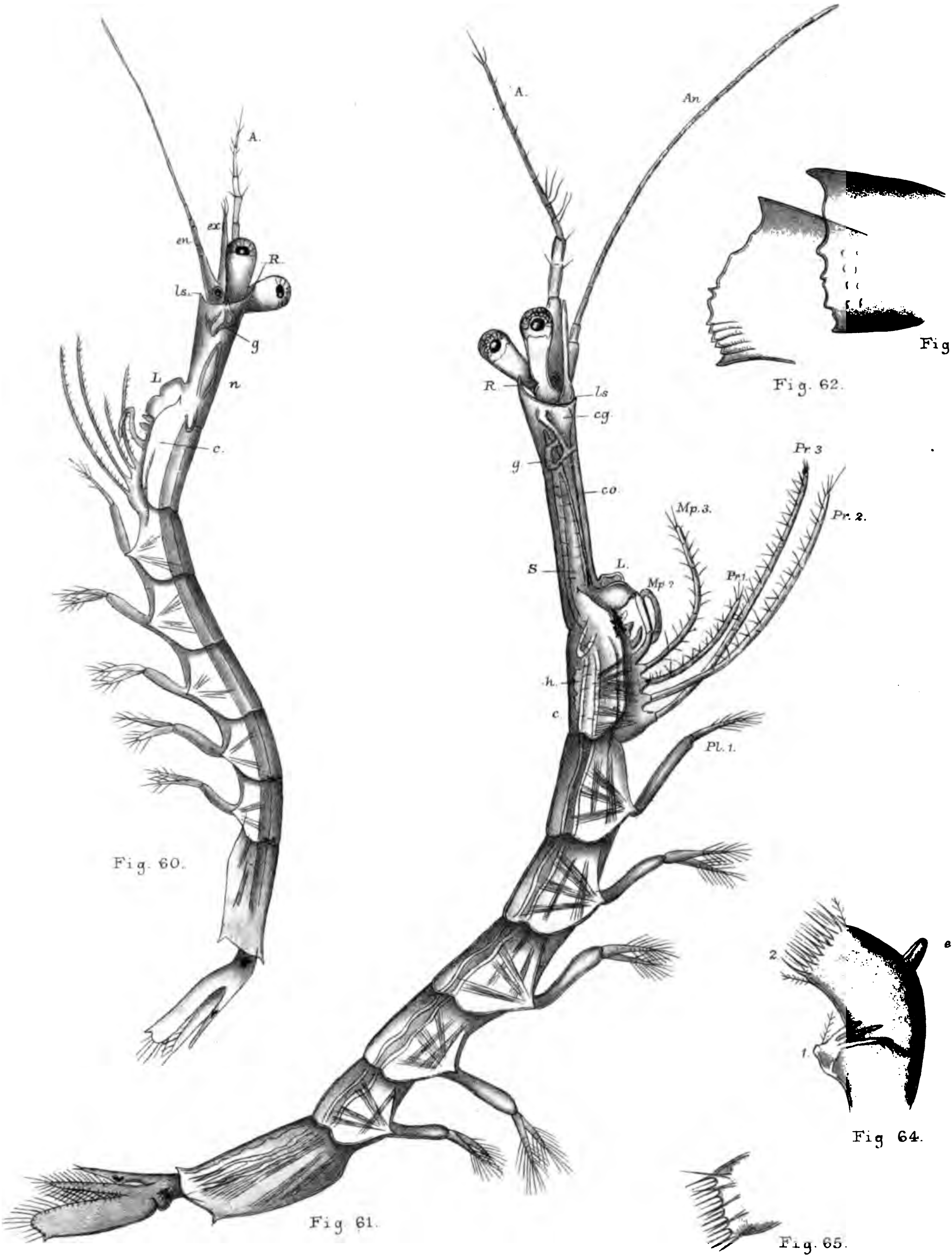


Fig. 60.

Fig 61.

Fig. 62.

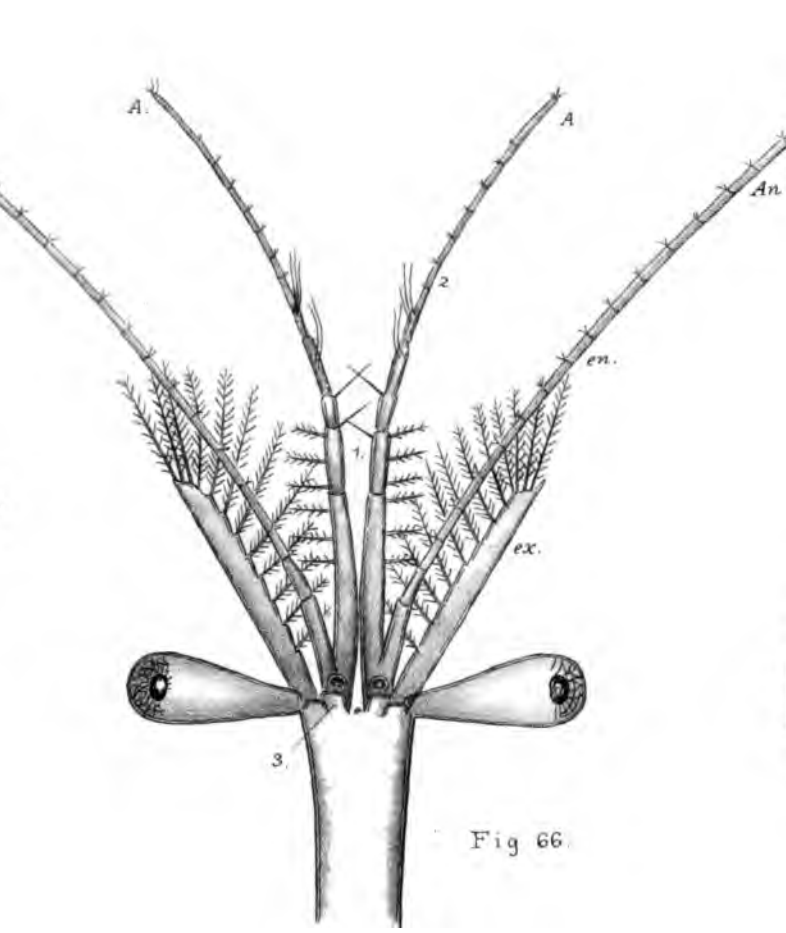
Fig. 63.

Fig 64.

Fig. 65.

W.K.B. ad nat del.

West Newman & Co. lith.



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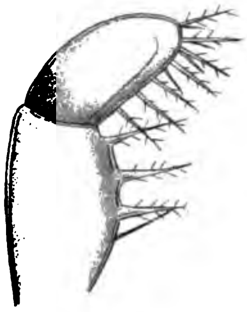

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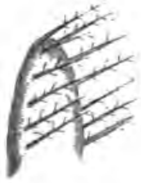

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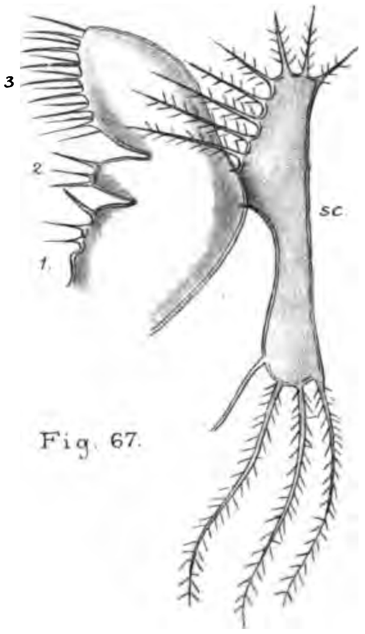

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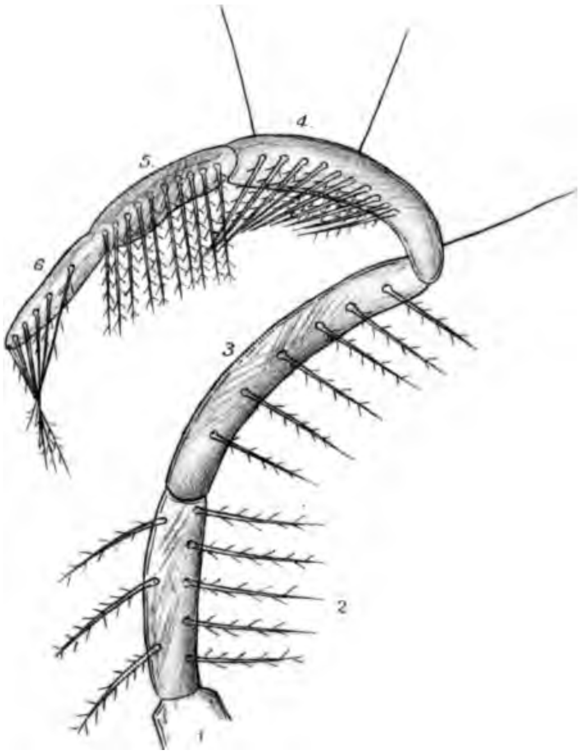
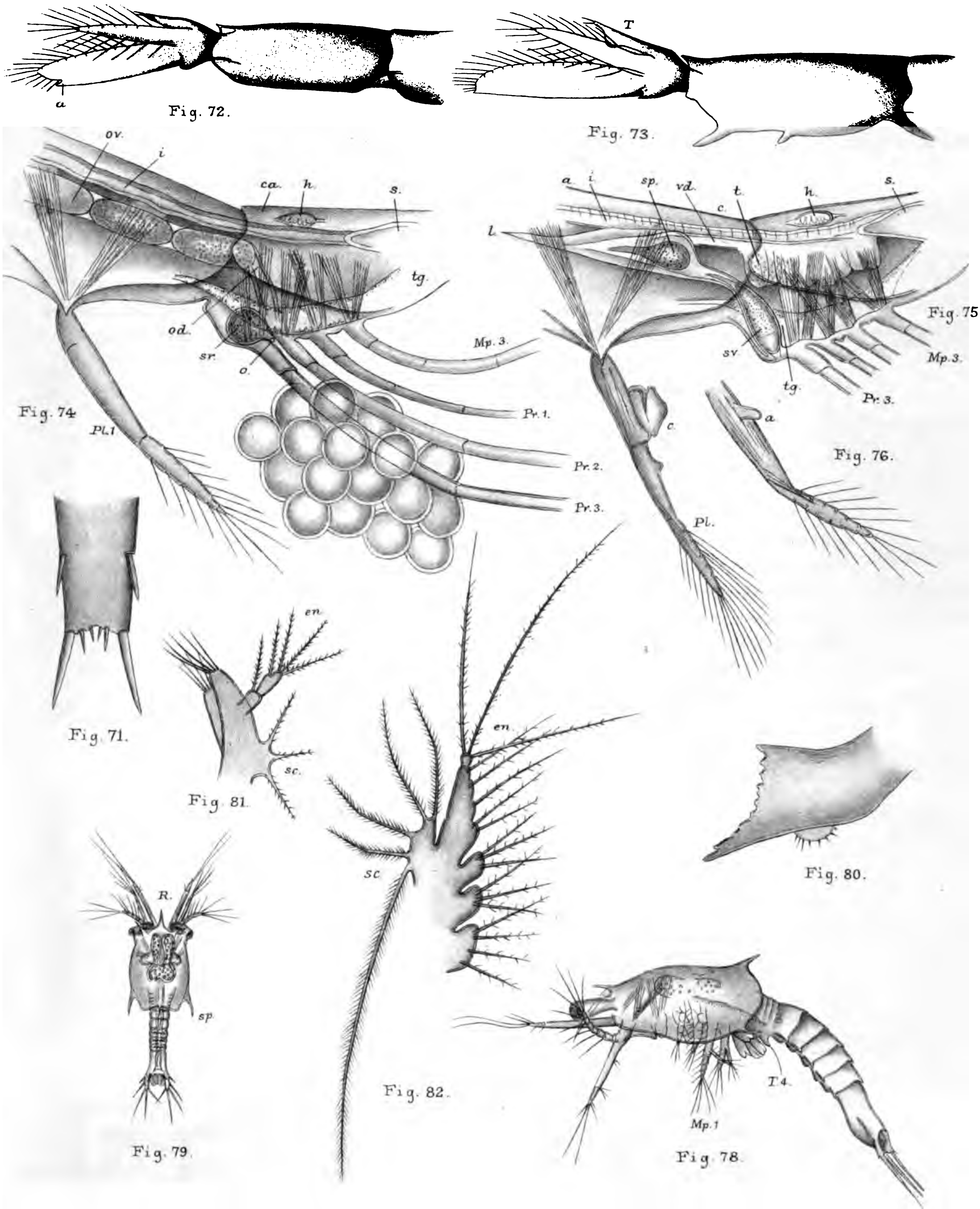


Fig. 70.

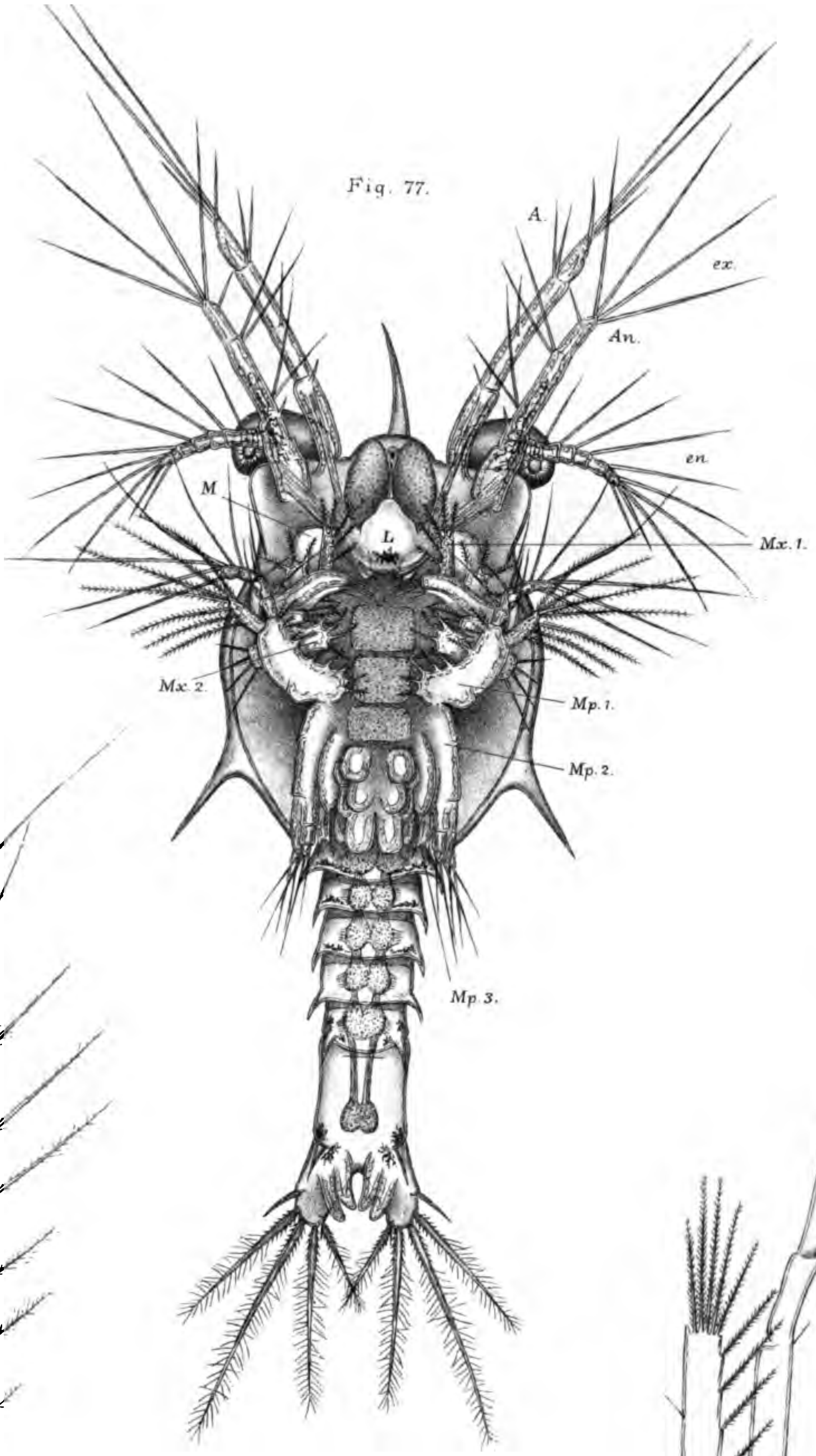
W.K.B. ad nat. del.

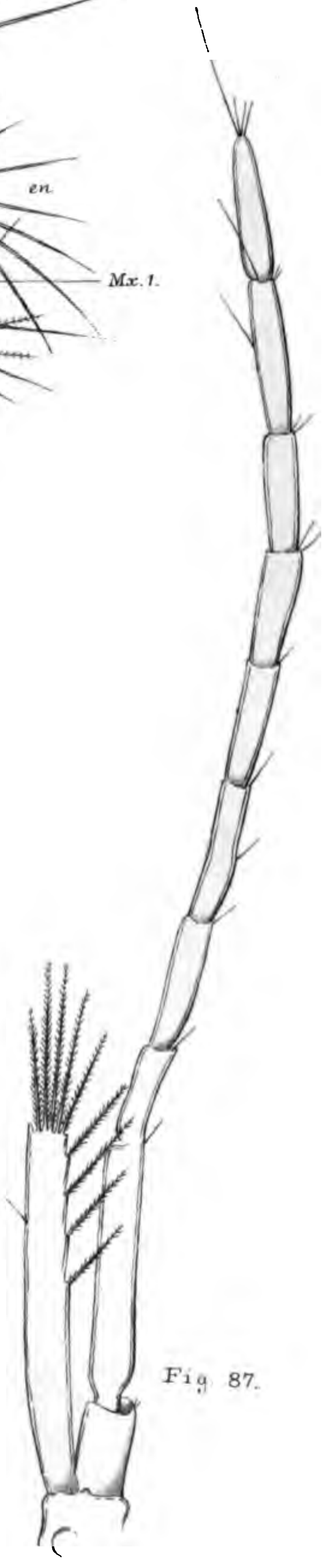
West Newman & Co lith.



W.K.B. ad nat. del.

West Newman & C°. lith.





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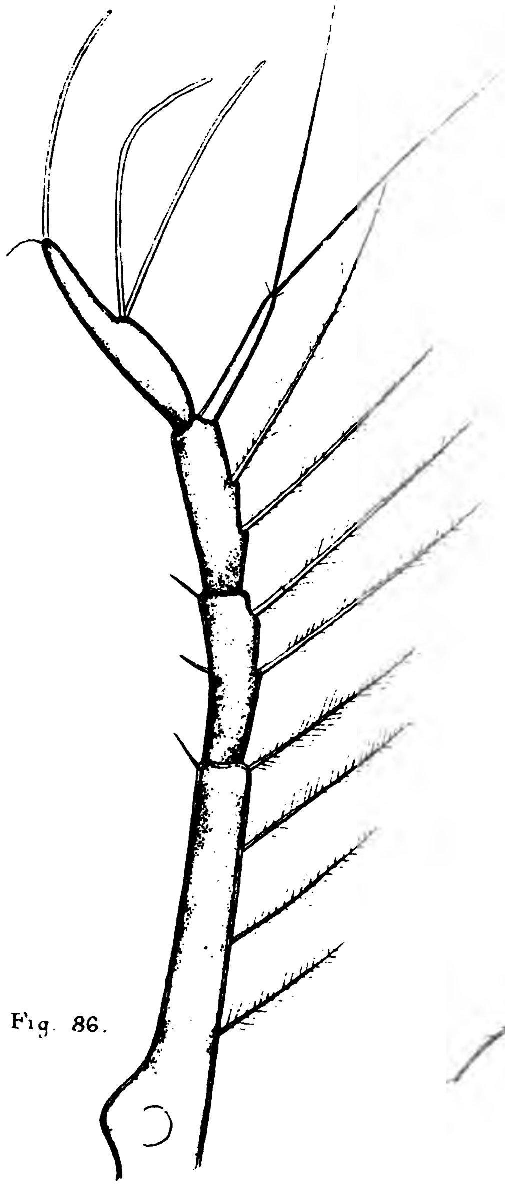

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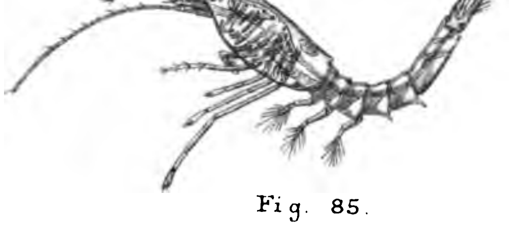
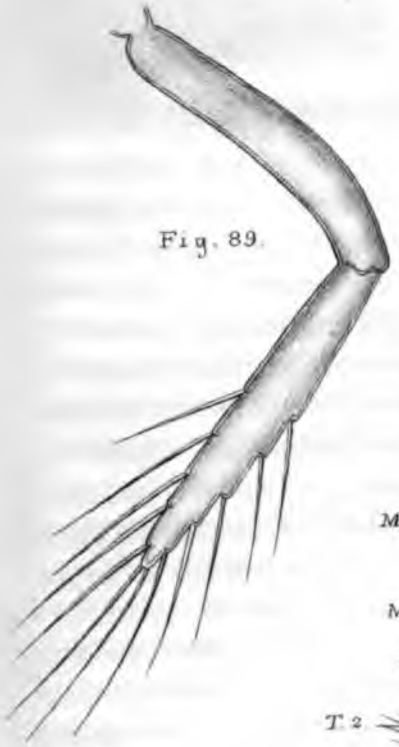


Fig. 85.

W.K.B. ad nat. del.

West Newman & Co lith.



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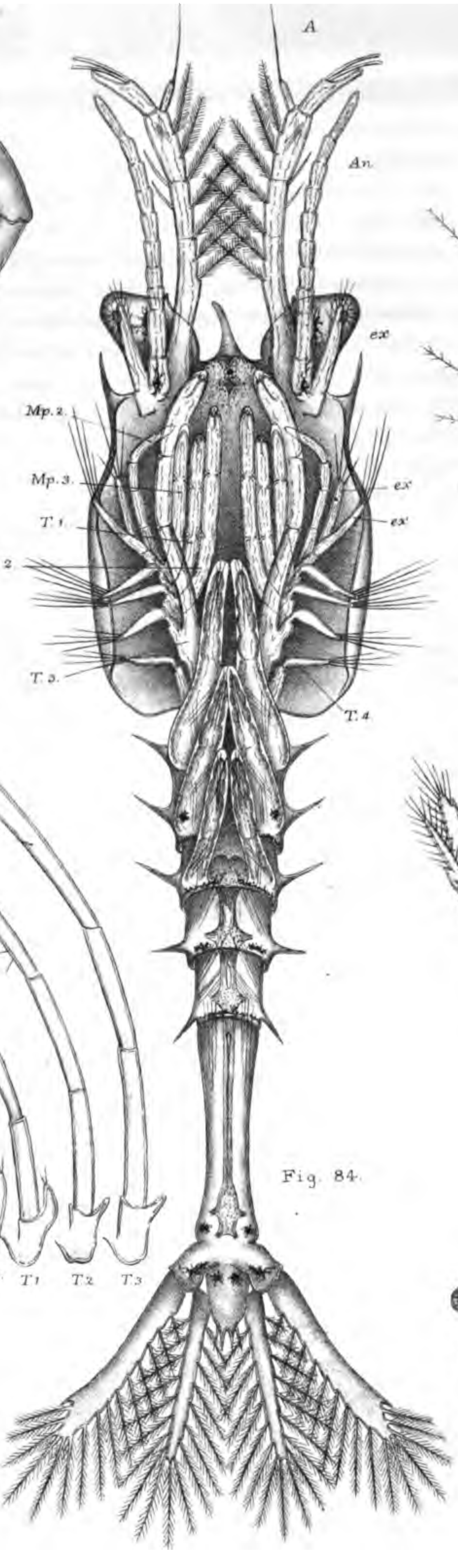

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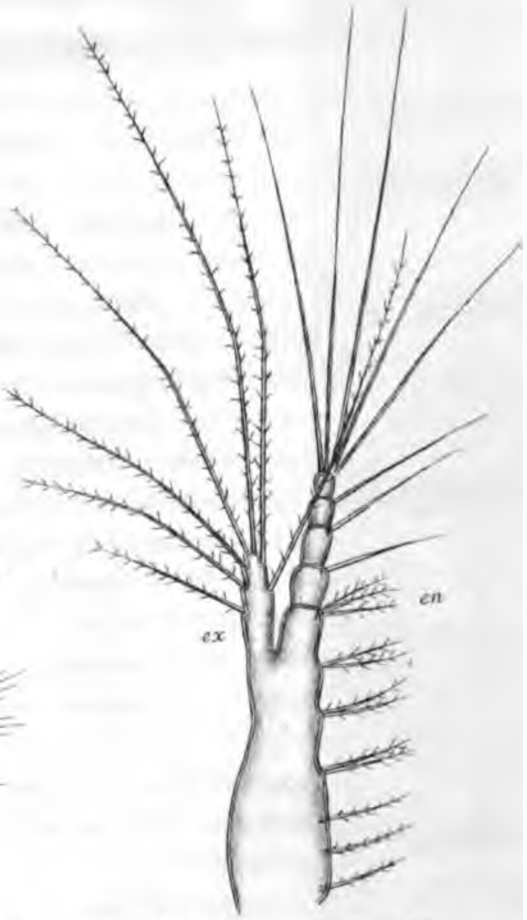

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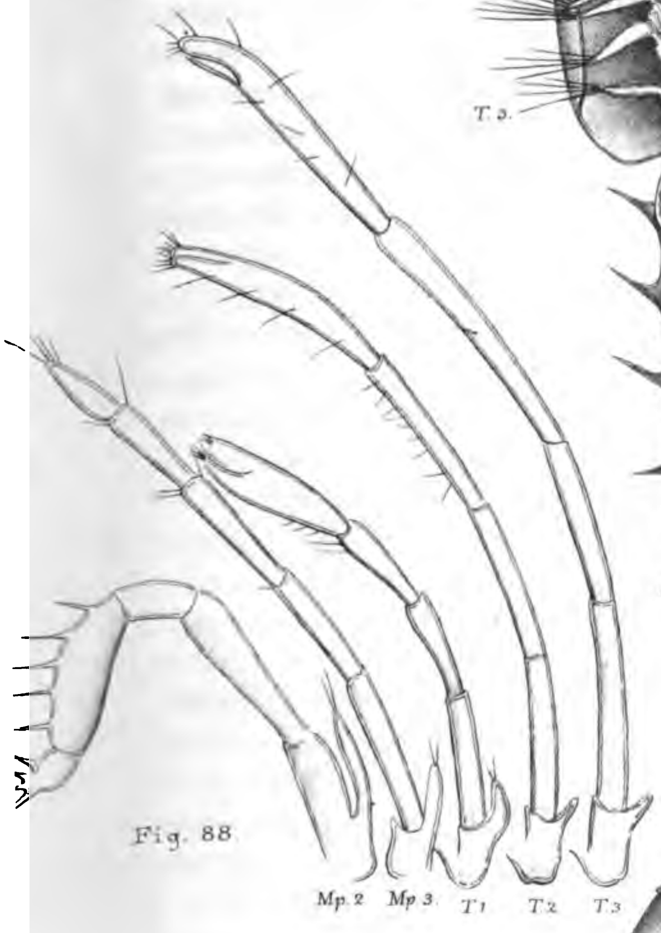


Fig. 88

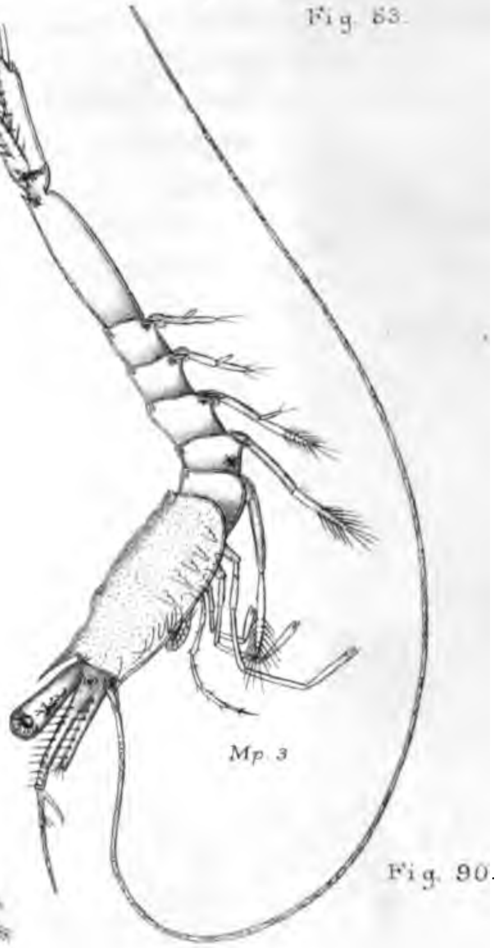
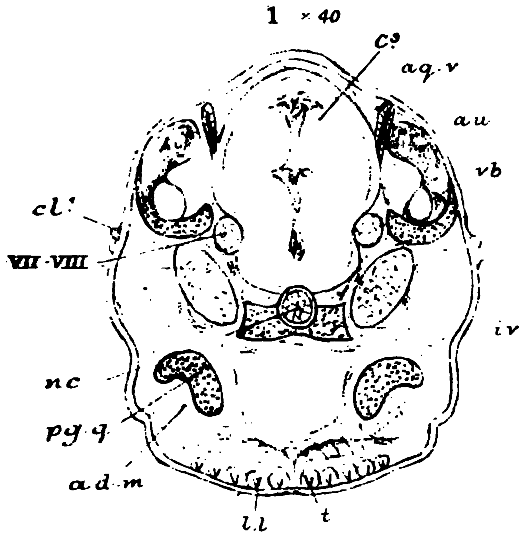


Fig. 90.

L.B. ad nat. del.

West Newman & Co lith.

1 × 40



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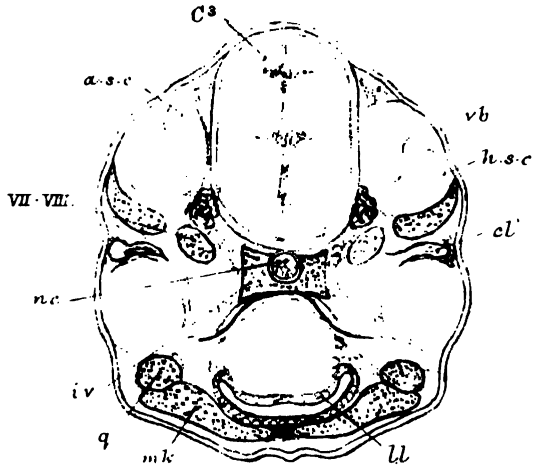
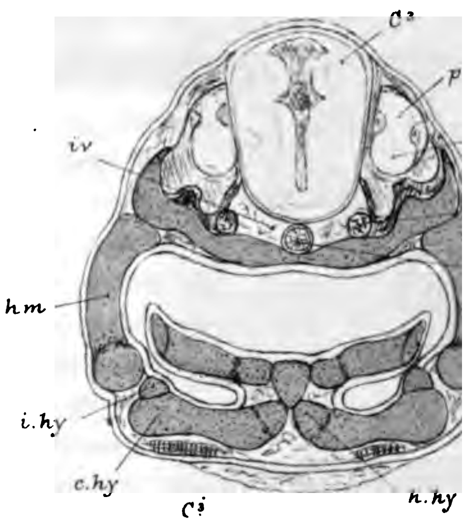
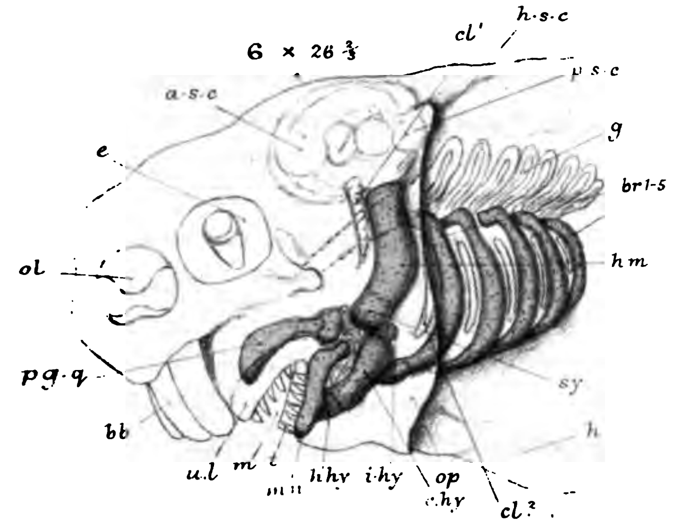


3 × 40



6 × 26⅔



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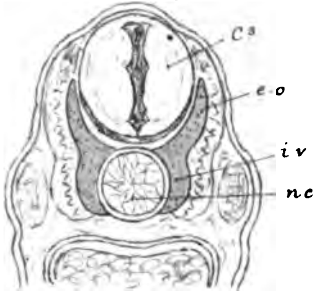

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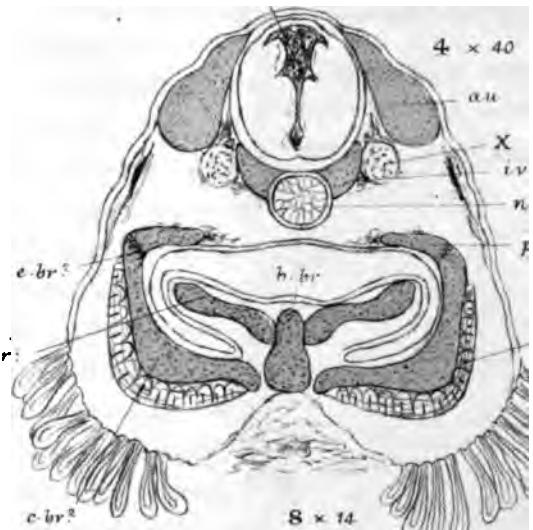
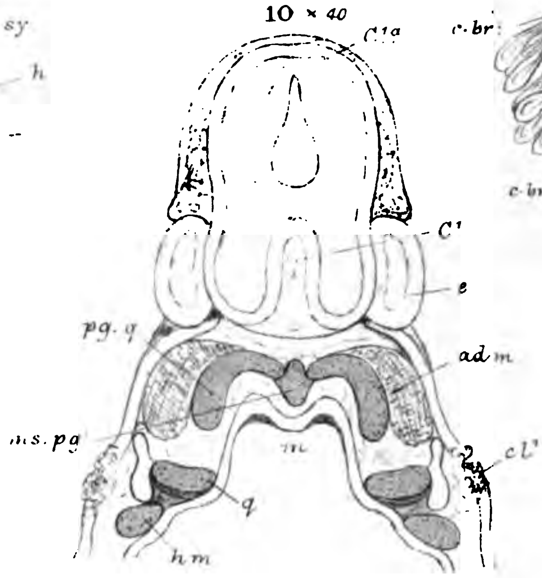


10 × 40



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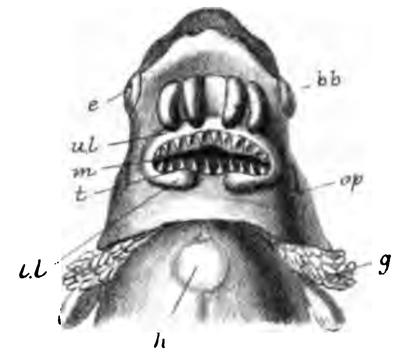
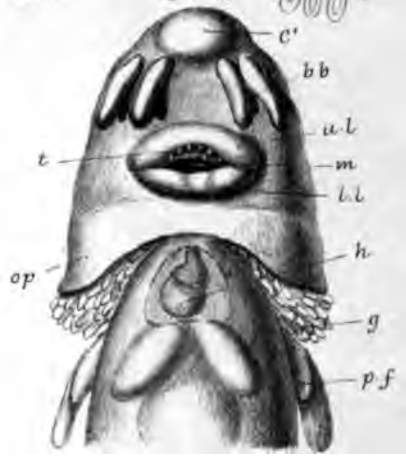


8 × 14



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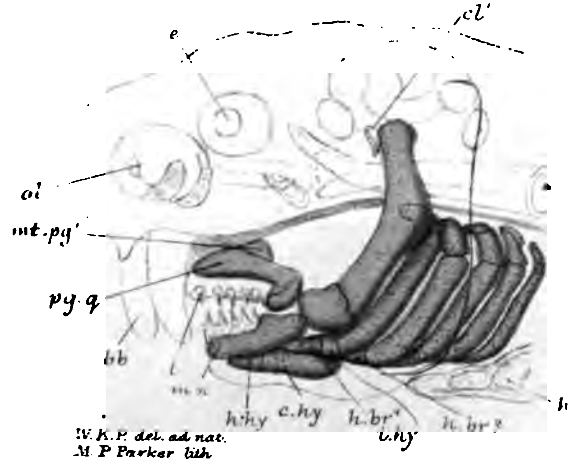

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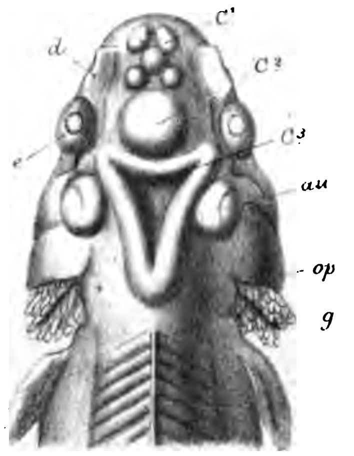
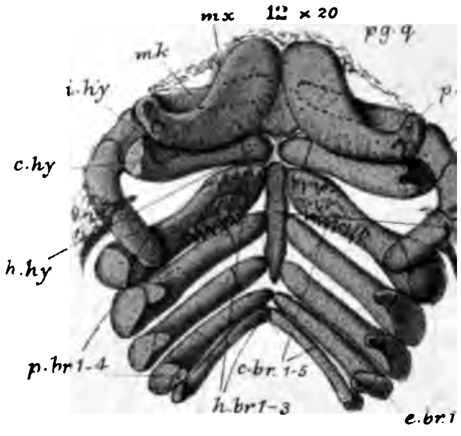


12 × 20

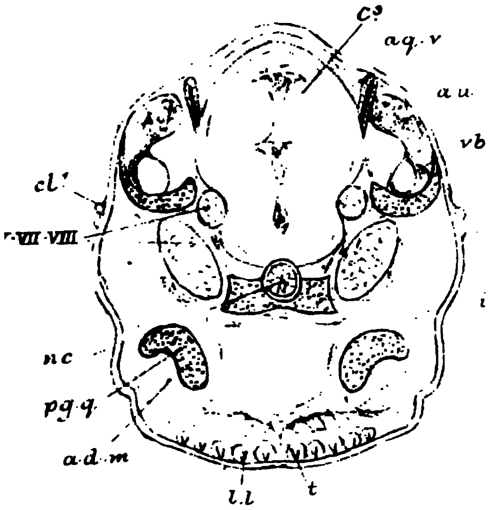


W. K. P. del. ad nat.
M. P. Parker lith.

ACIPENSER.

West Newman & Co.

1 × 40



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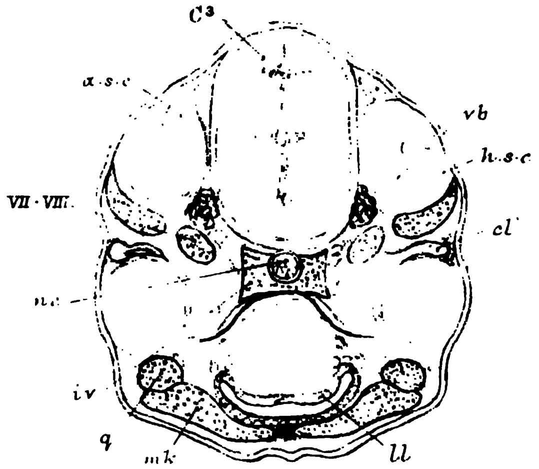

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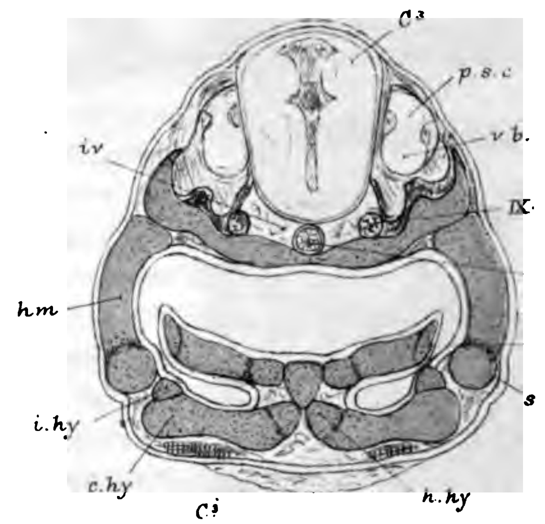
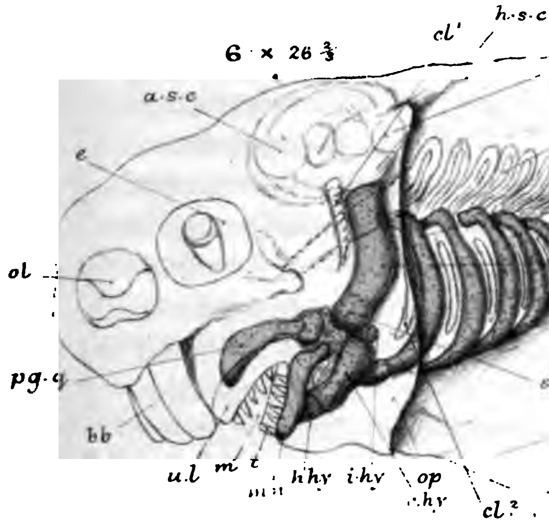


6 × 26 2/3



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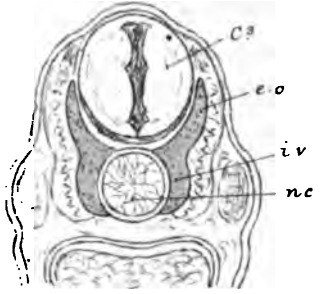

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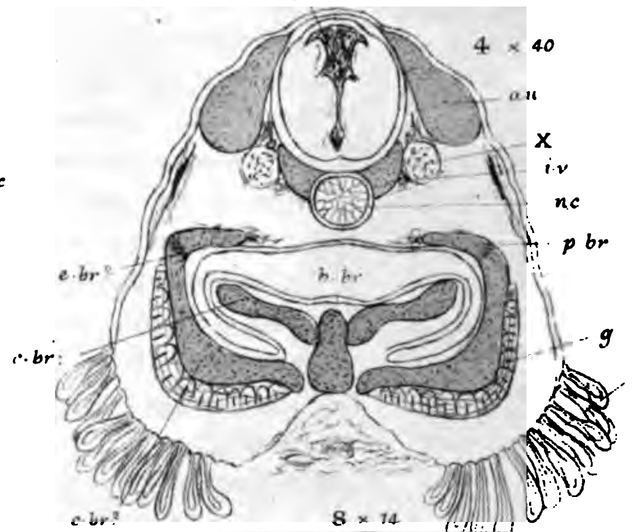

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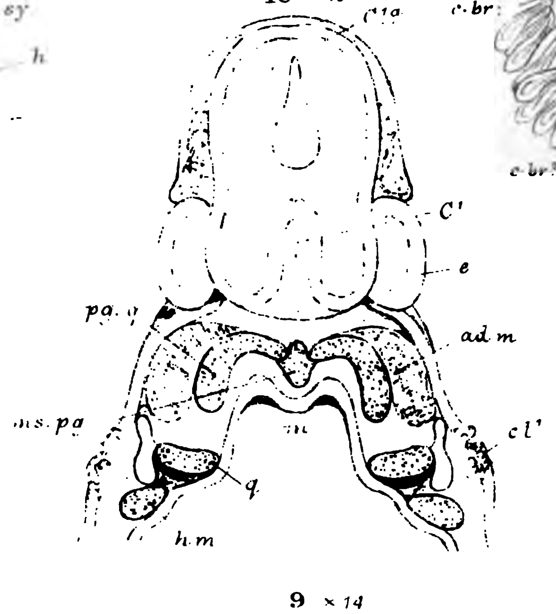
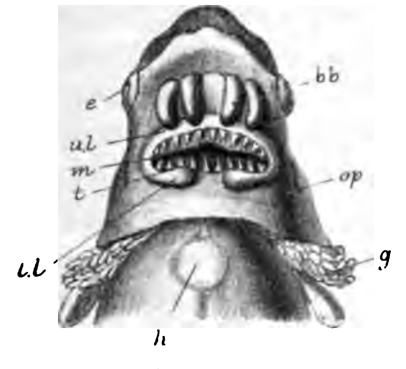


7 × 14



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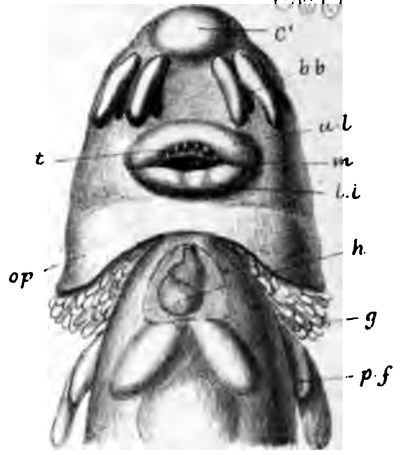

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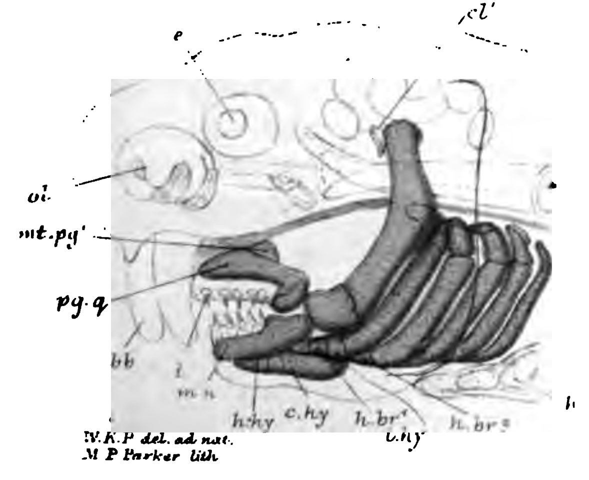
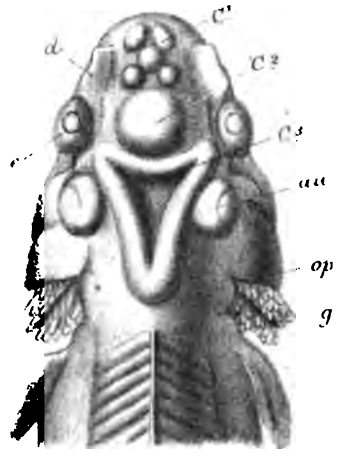


9 × 14



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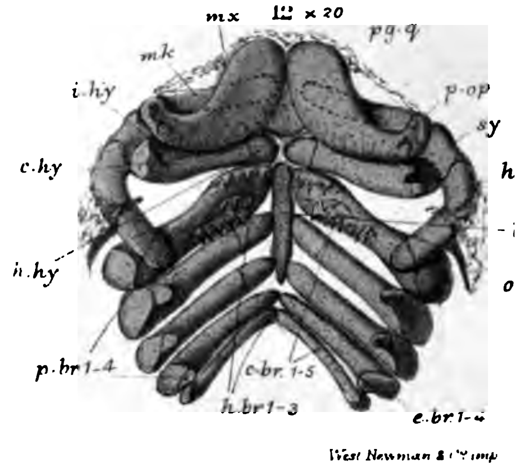
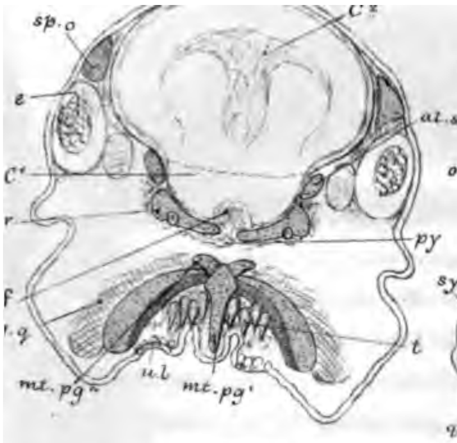
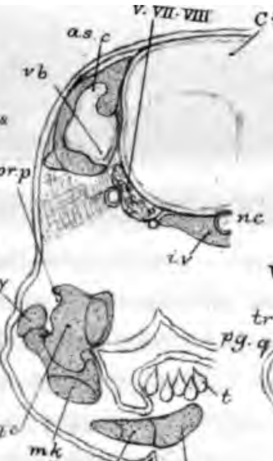


W.K.P. del. ad nat.
M P Parker lith.

ACIPENSER.

West Newman & Co imp.





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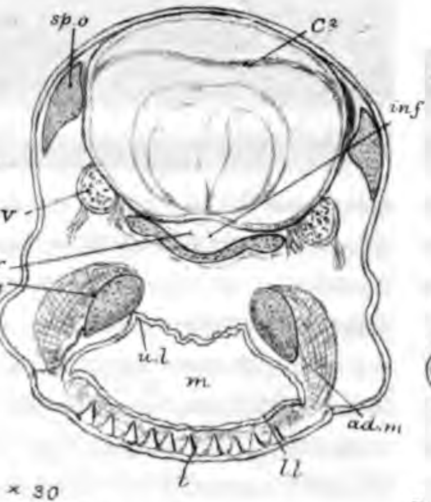

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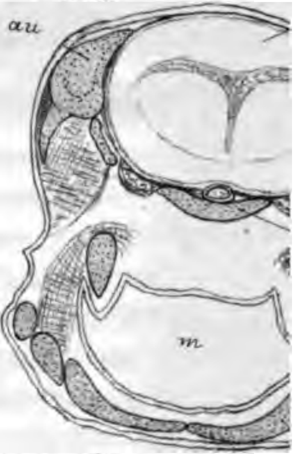

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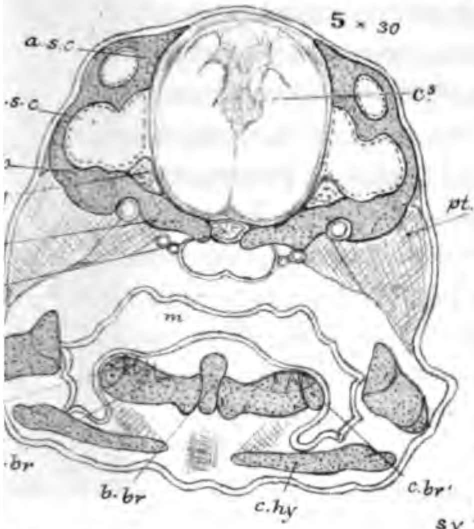

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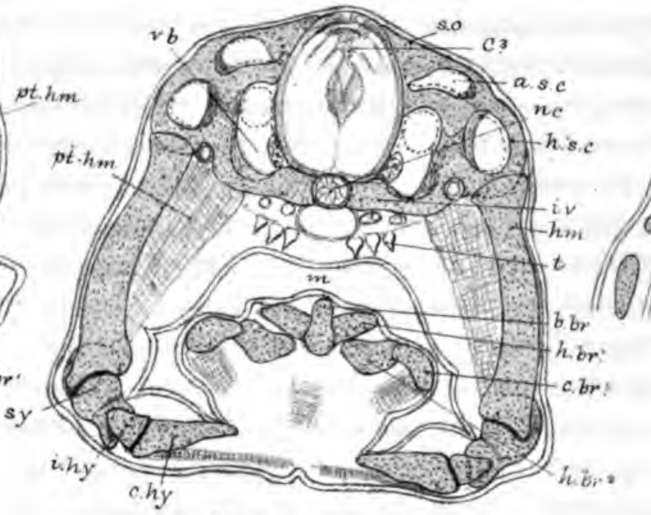

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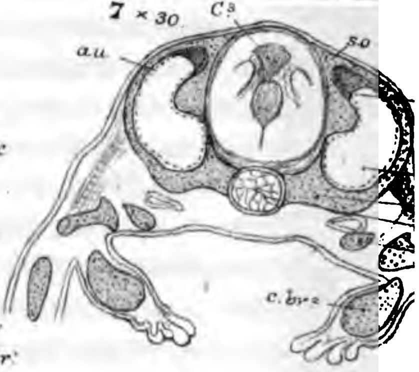

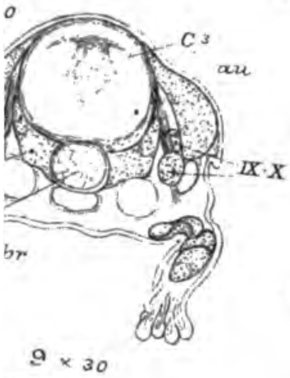

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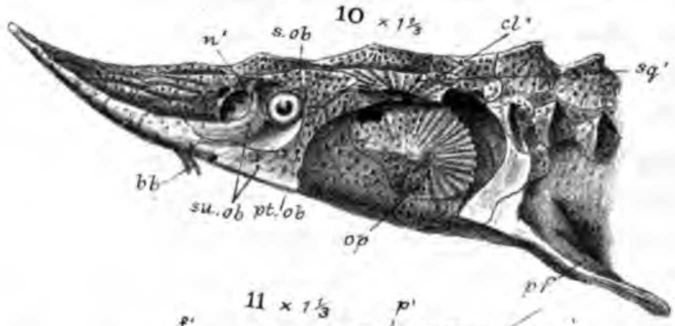

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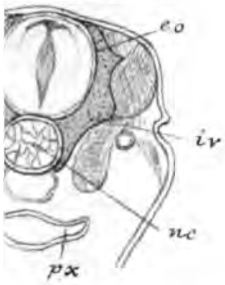

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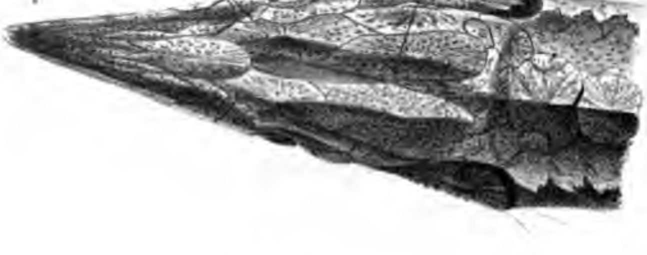
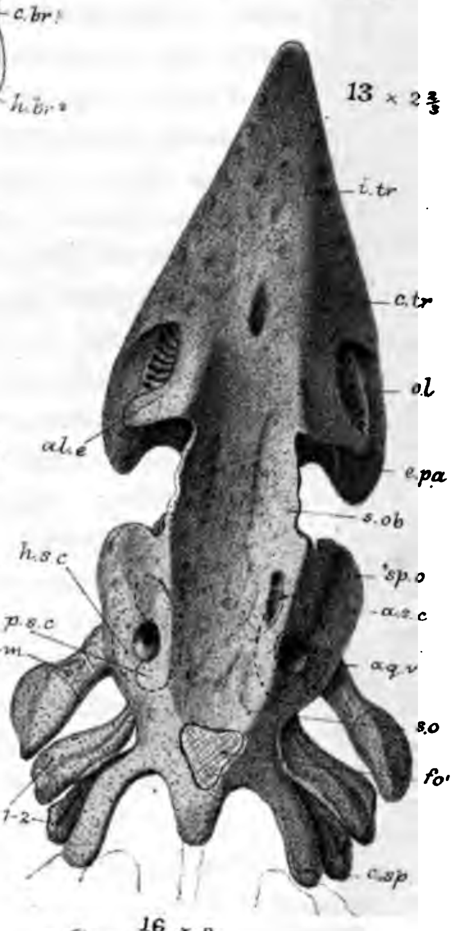
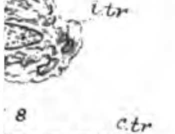


13 × 2⅔





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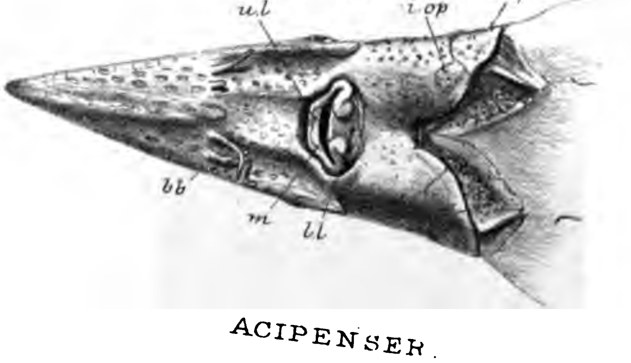
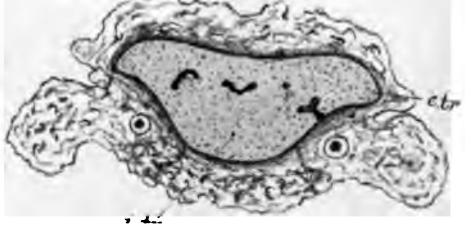
12 × 1⅓



16 × 8



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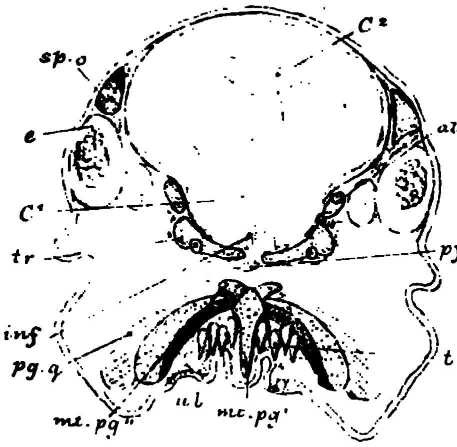
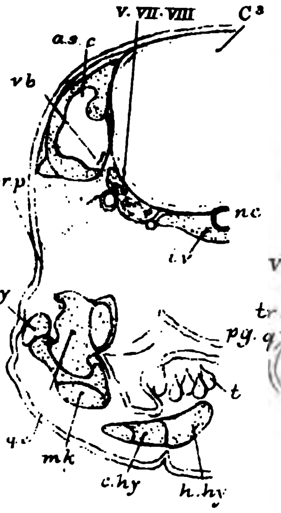
Phil. Trans. 1882. Plate 15.

1 × 30



4 × 30



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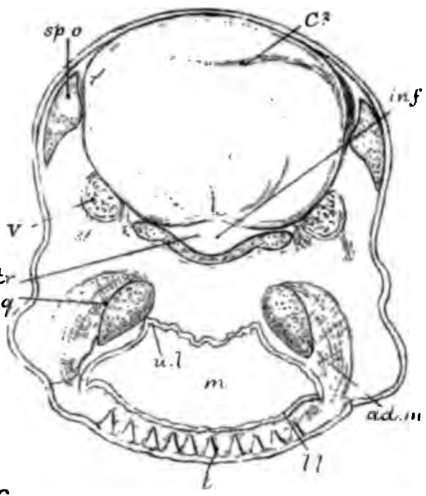

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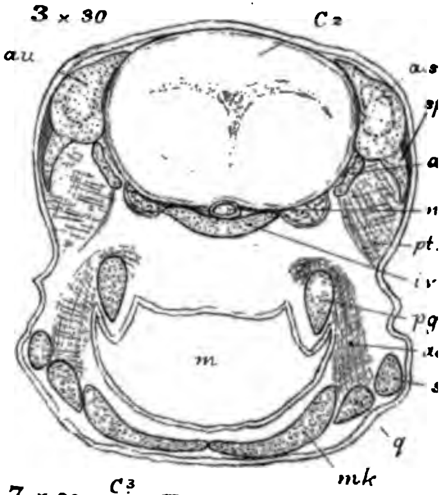

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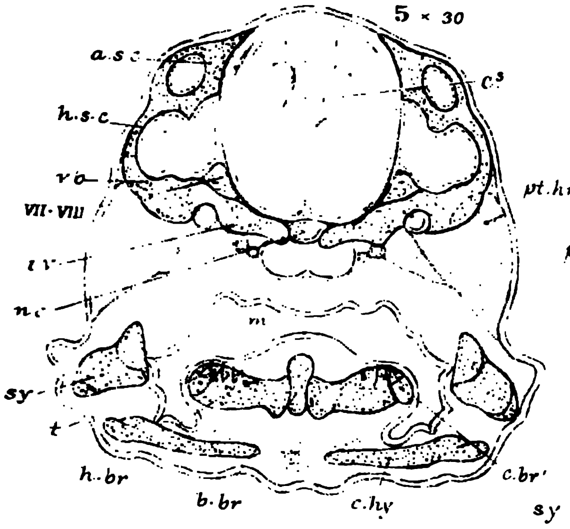

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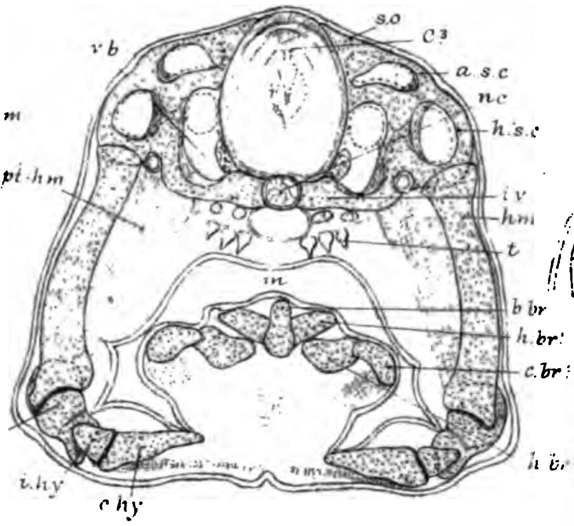
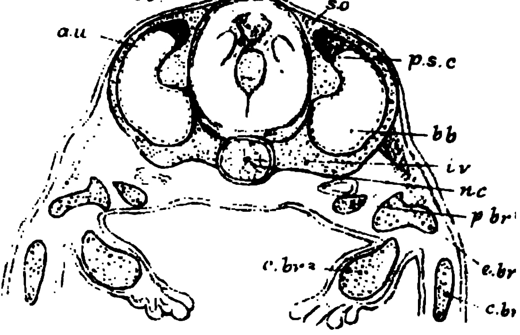
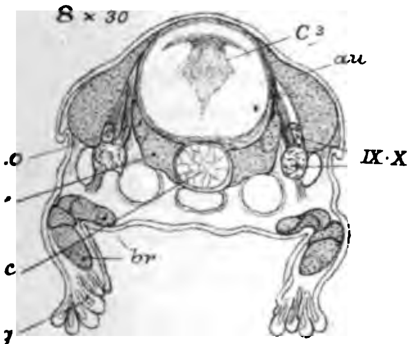


7 × 30



8 × 30



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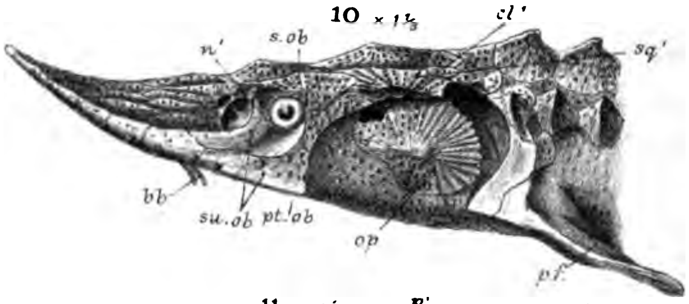

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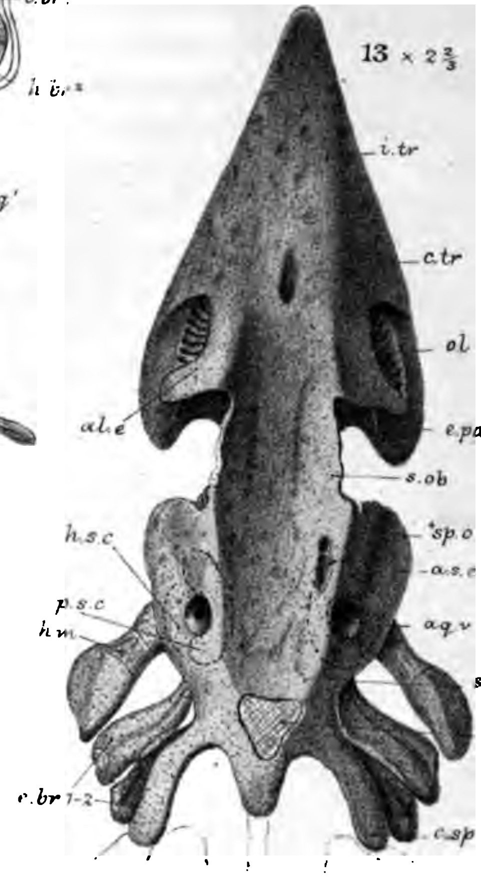
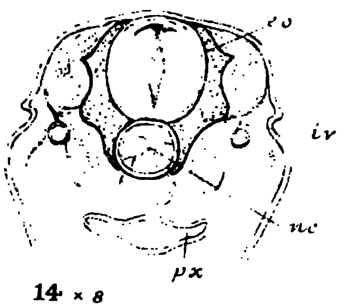
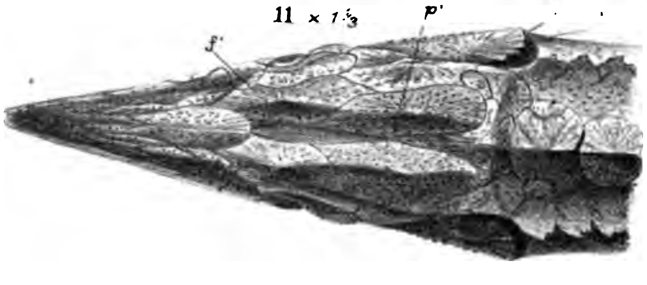


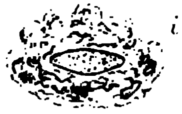
9 × 30



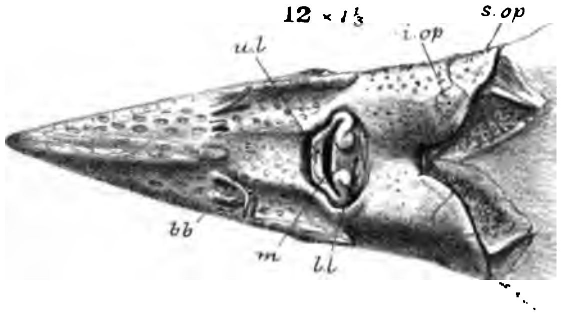
11 × 1⅓



14 × 8



12 × 1⅓



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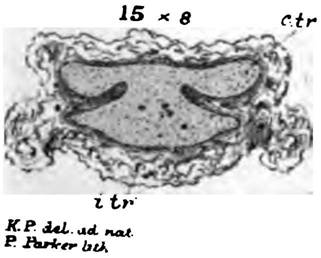

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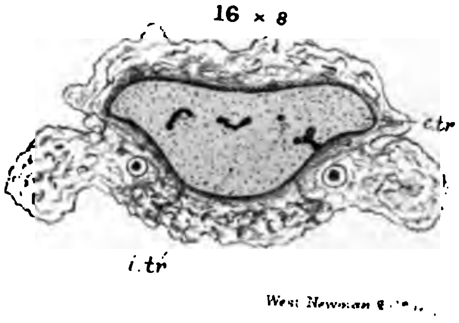

K.P. del. ad nat.
P. Parker lith.

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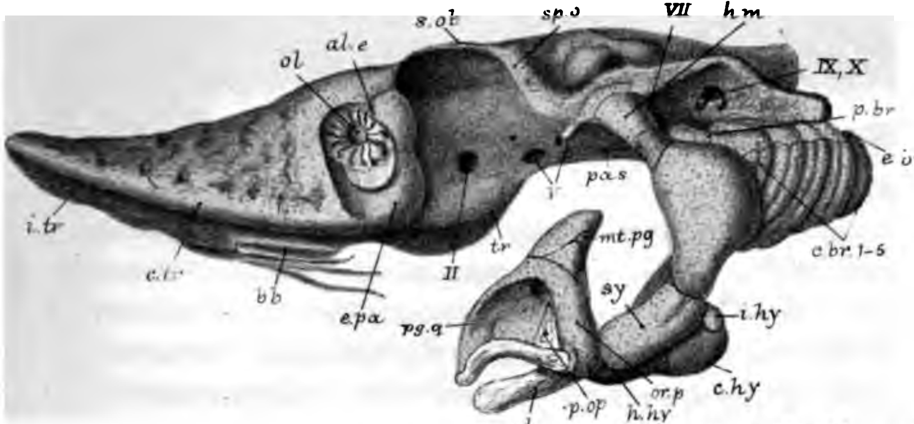
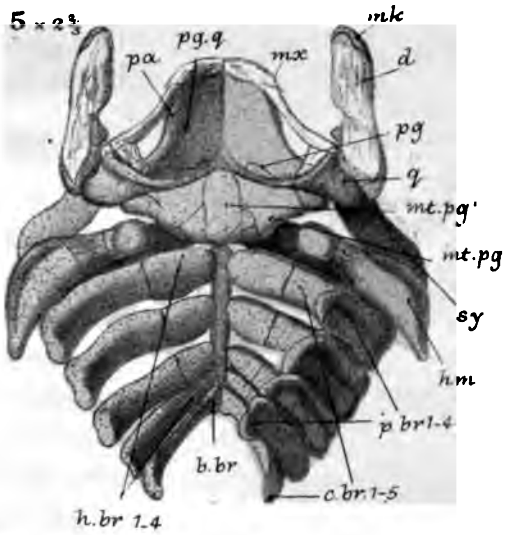
West Newman &

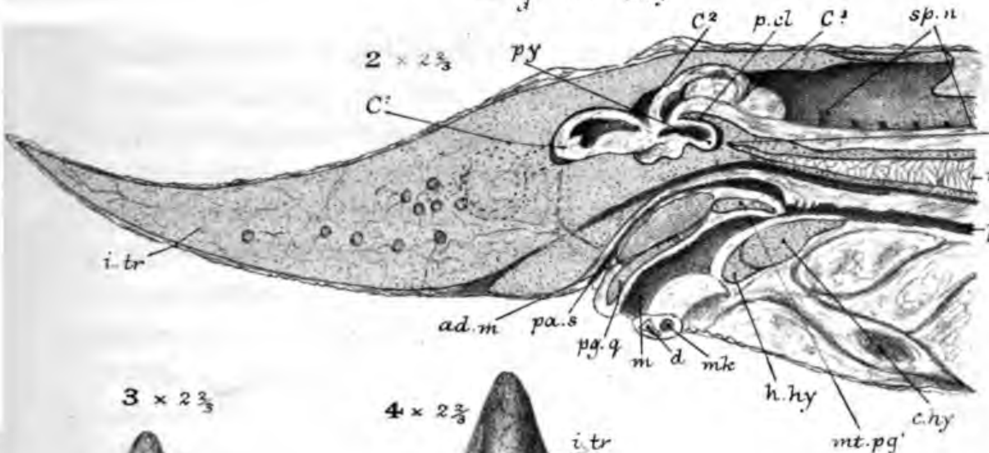
1 × 2⅔



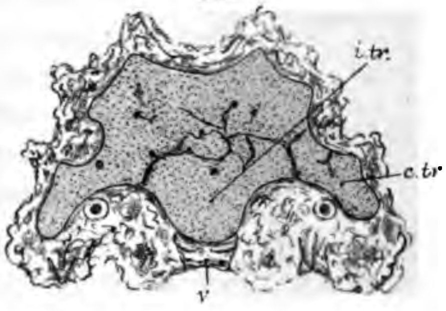
5 × 2⅔



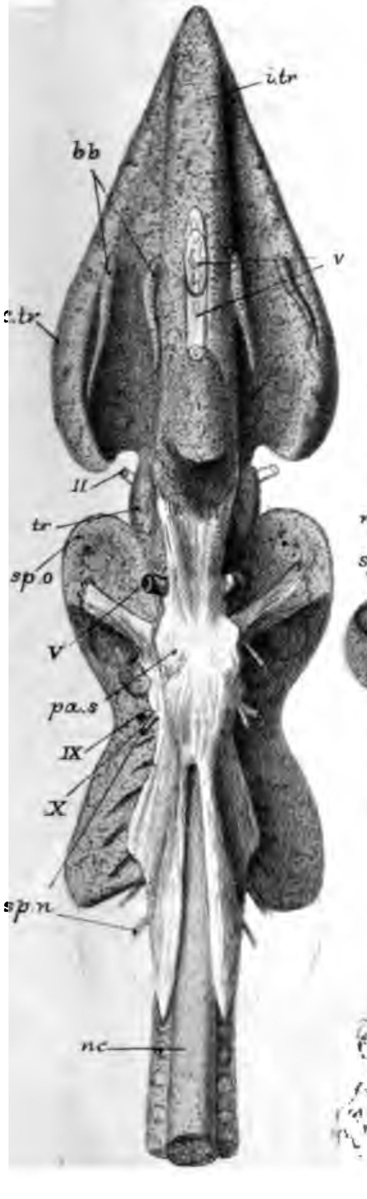
2 × 2⅔



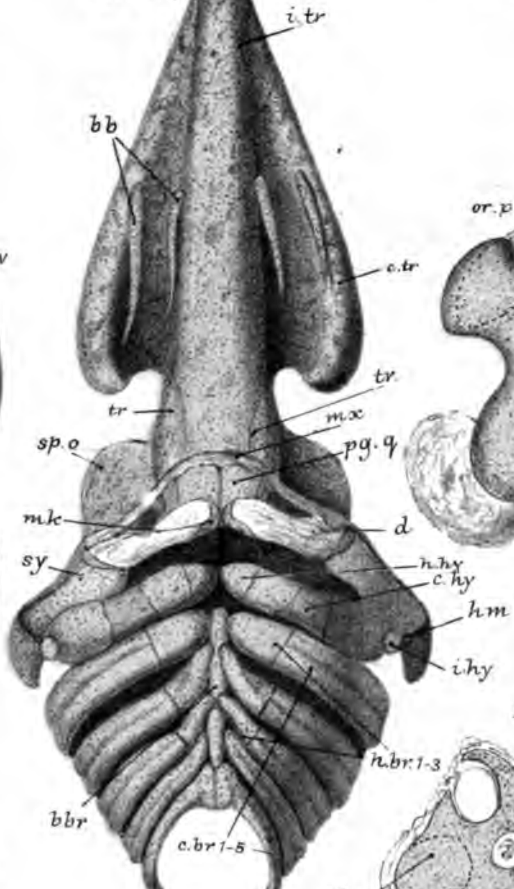
9. × 4



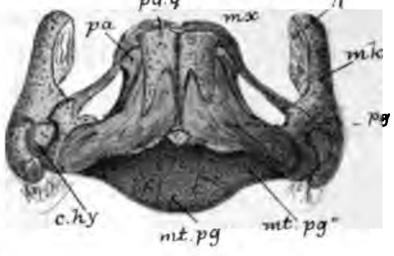
3 × 2⅔



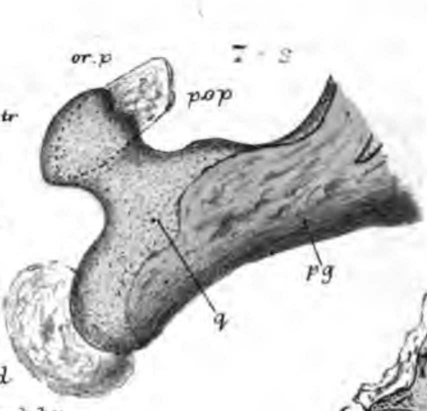
4 × 2⅔



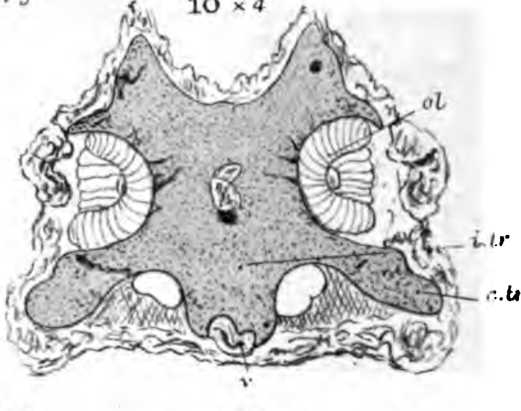
6 × 2⅔



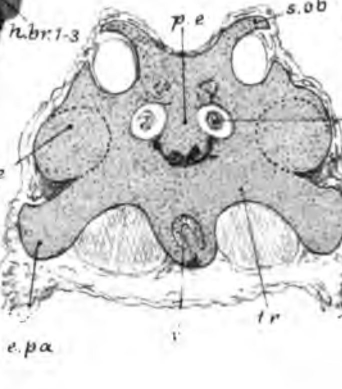
7 × 8



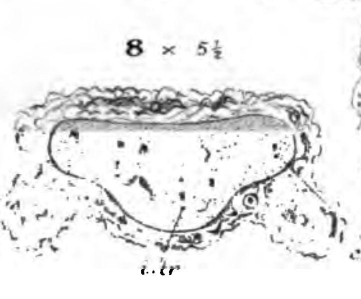
10 × 4



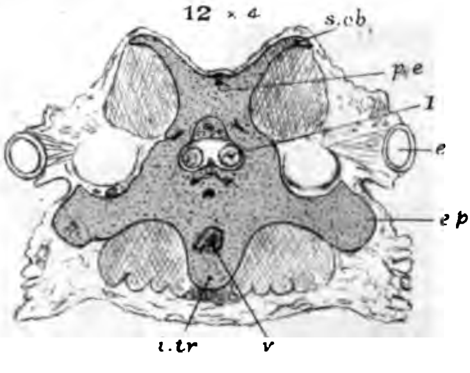
11. × 4



8 × 5½



12 × 4

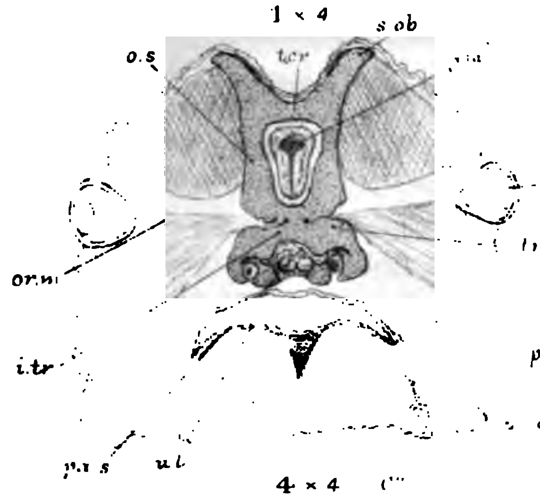


W. K. P. del. ad nat.
M. P. Parker lith.

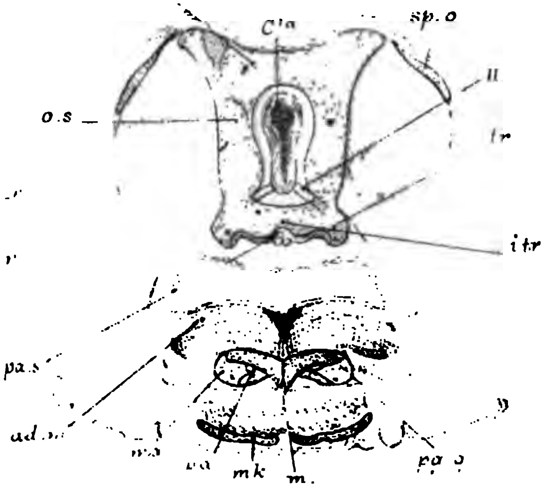
ACIPENSER.

West Newman & Co imp.

1 × 4



2 × 4



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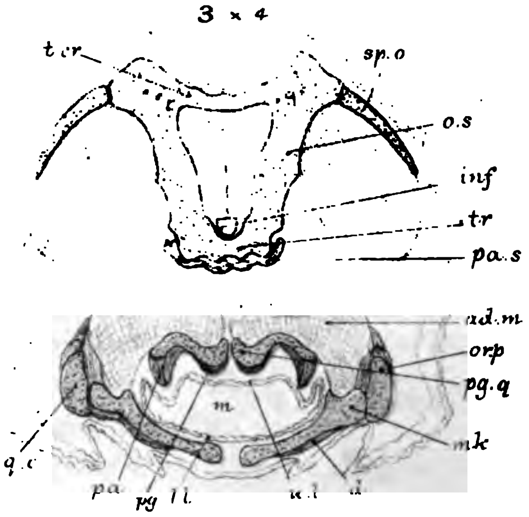
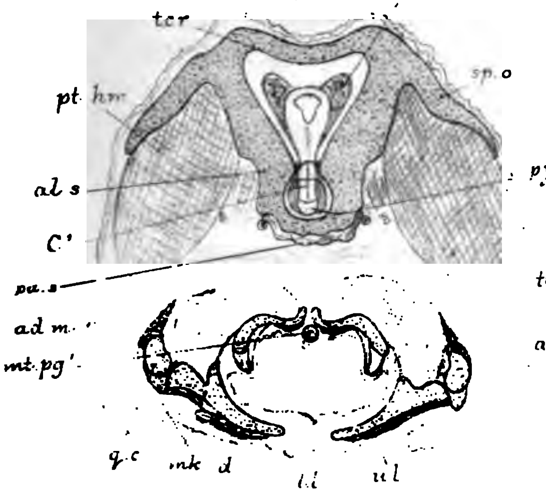
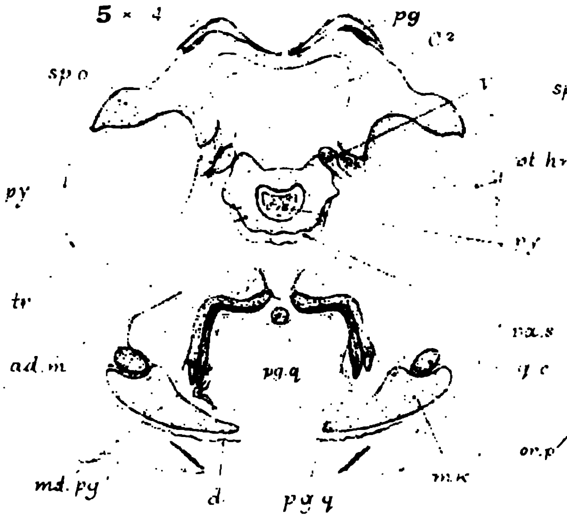


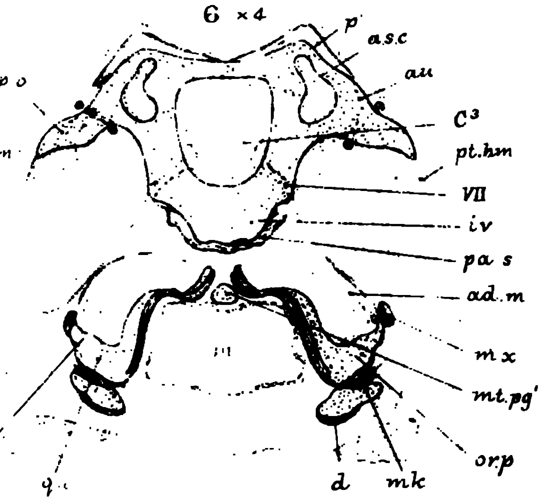
4 × 4



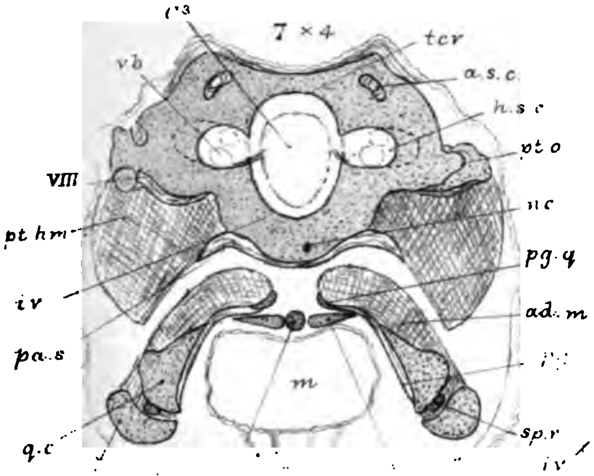
5 × 4



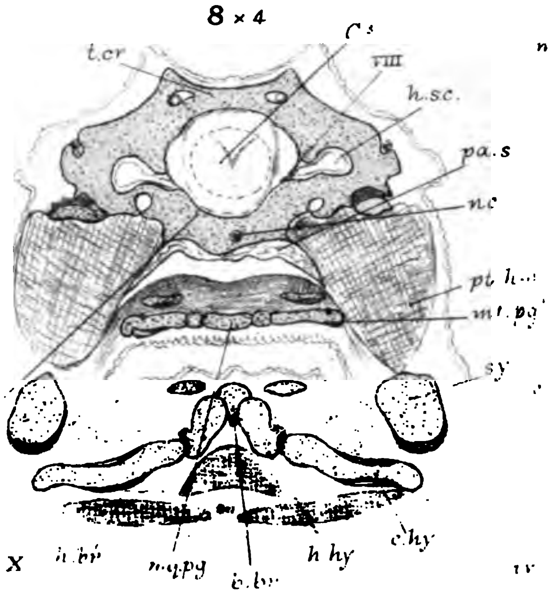
6 × 4



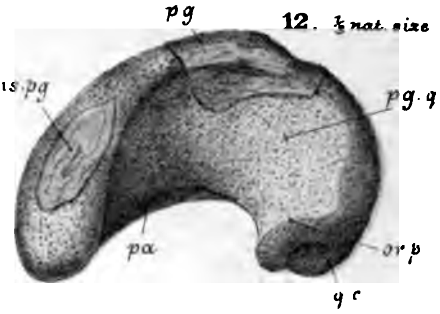
7 × 4



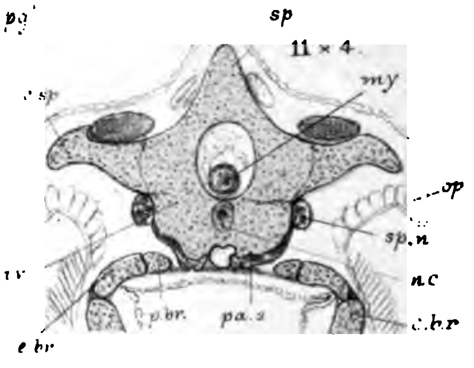
8 × 4



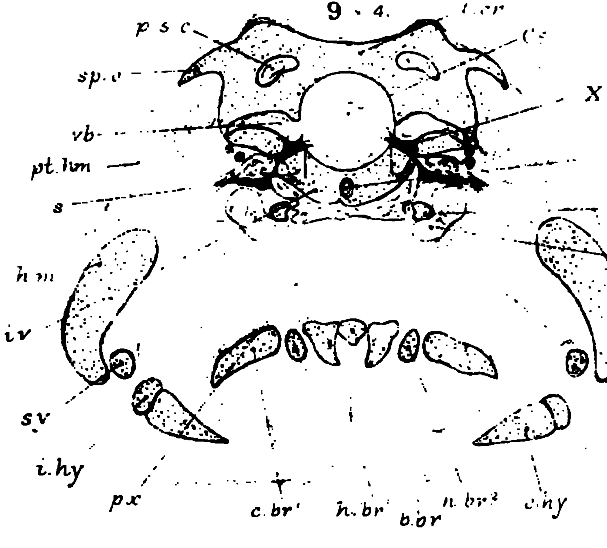
12. ½ nat. size



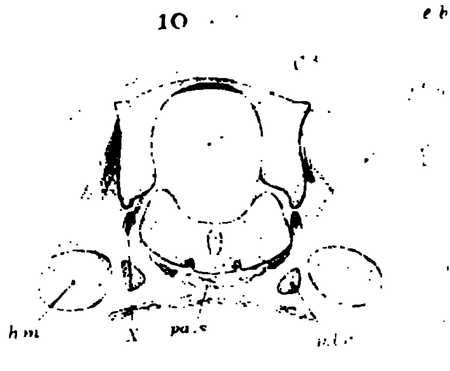
11 × 4



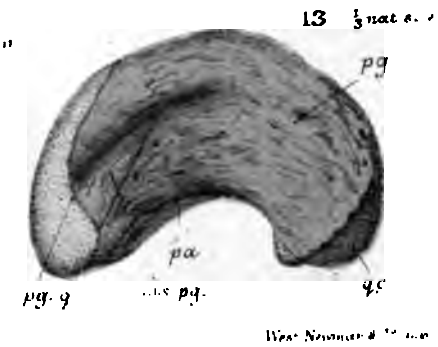
9 × 4.



10



13 ⅓ nat. s.

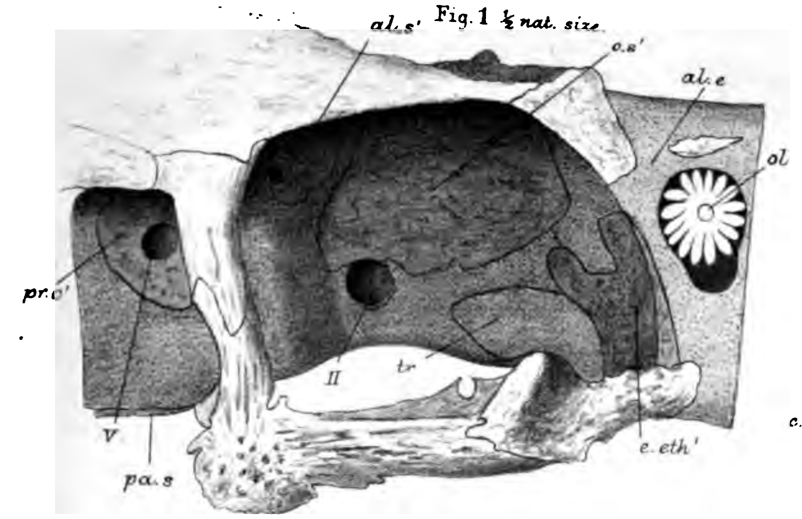


W. K. P. del. ad nat.
M. P. Parker lith.

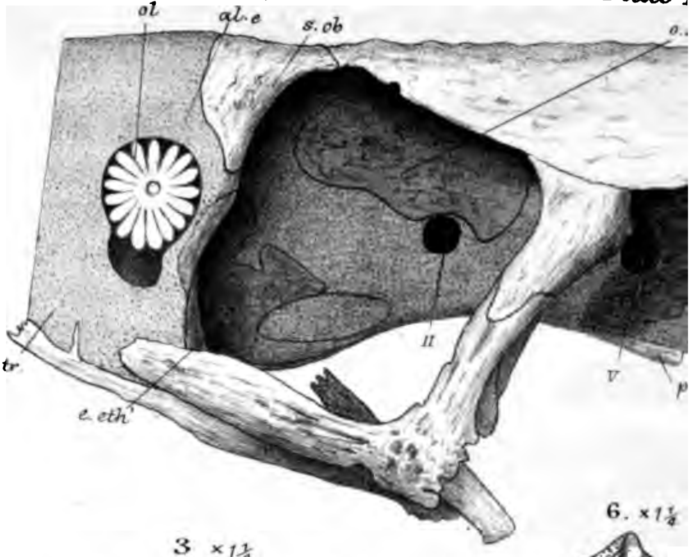
ACIPENSER.

West, Newman & Co. imp.

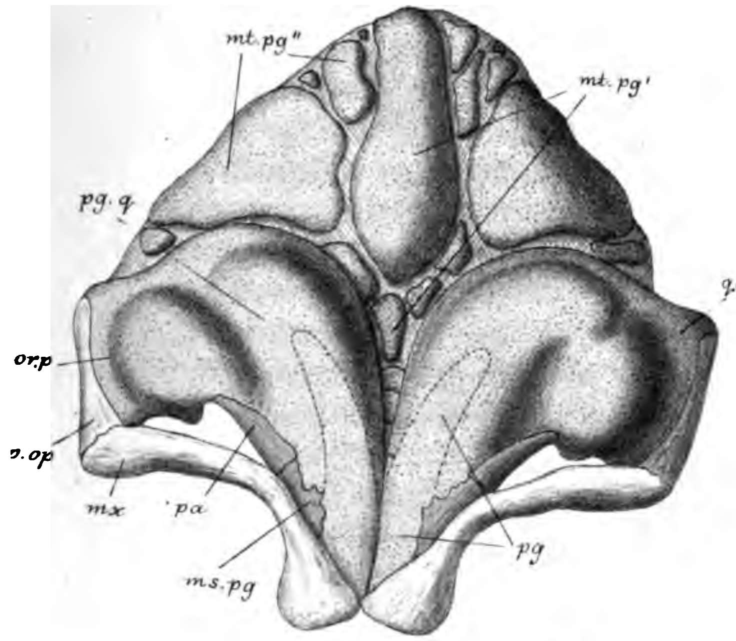
Fig. 1 ½ nat. size.



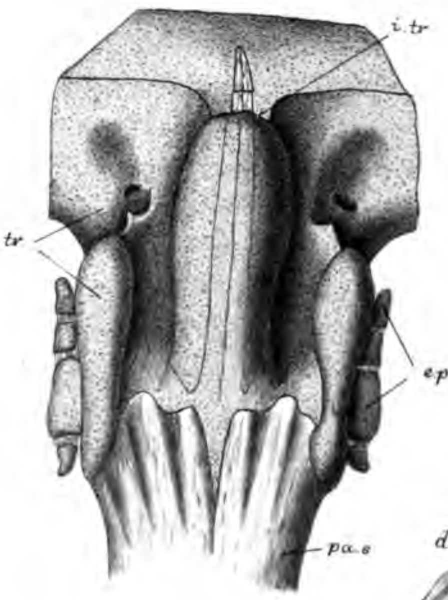
2. ½ nat. size.



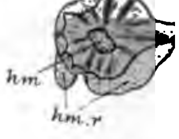
4 × 1¼



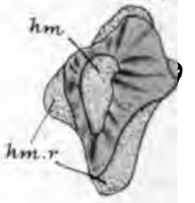
3 × 1¼



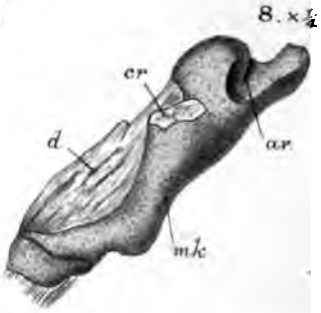
6. × 1¼



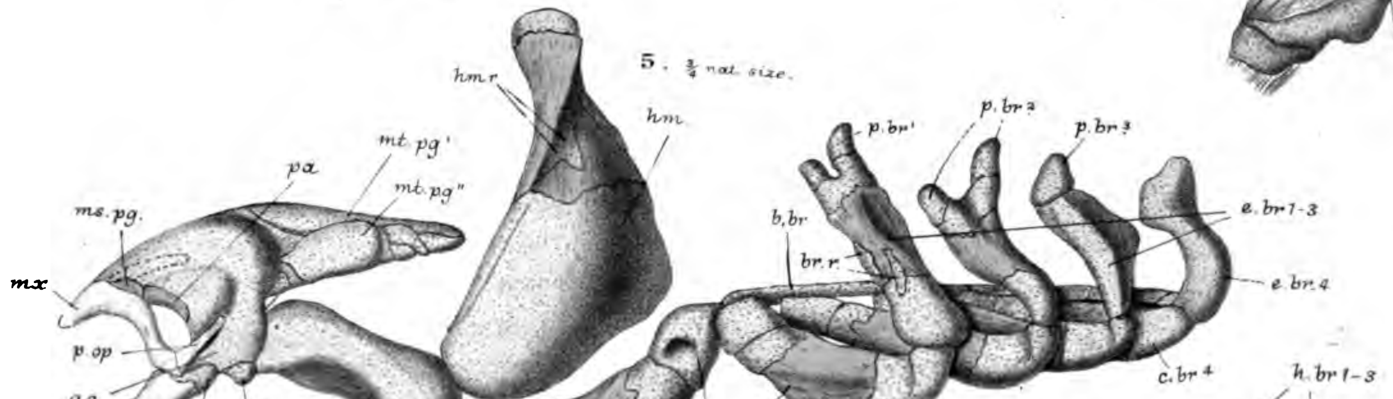
7. × 1



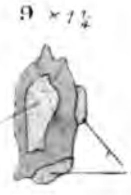
8. × ¾



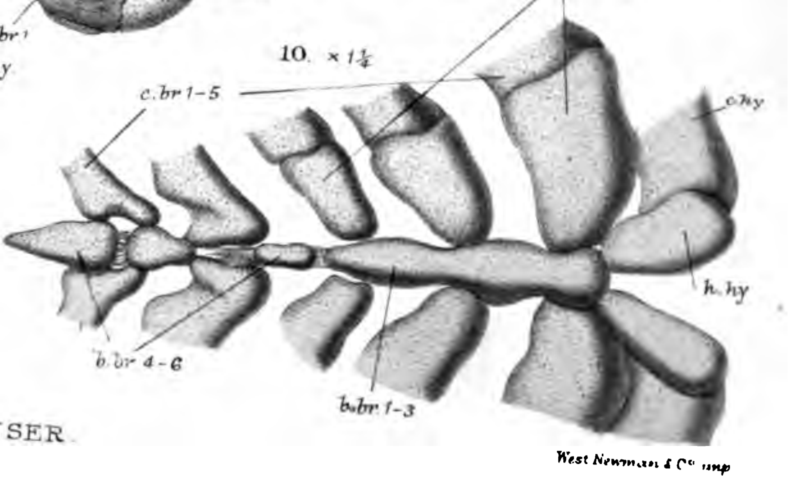
5. ¾ nat. size.



9 × 1¼



10. × 1¼



owes del. ad nat.
Parker lith.

ACIPENSER.

West Newman & Co imp.

Fig. 4.

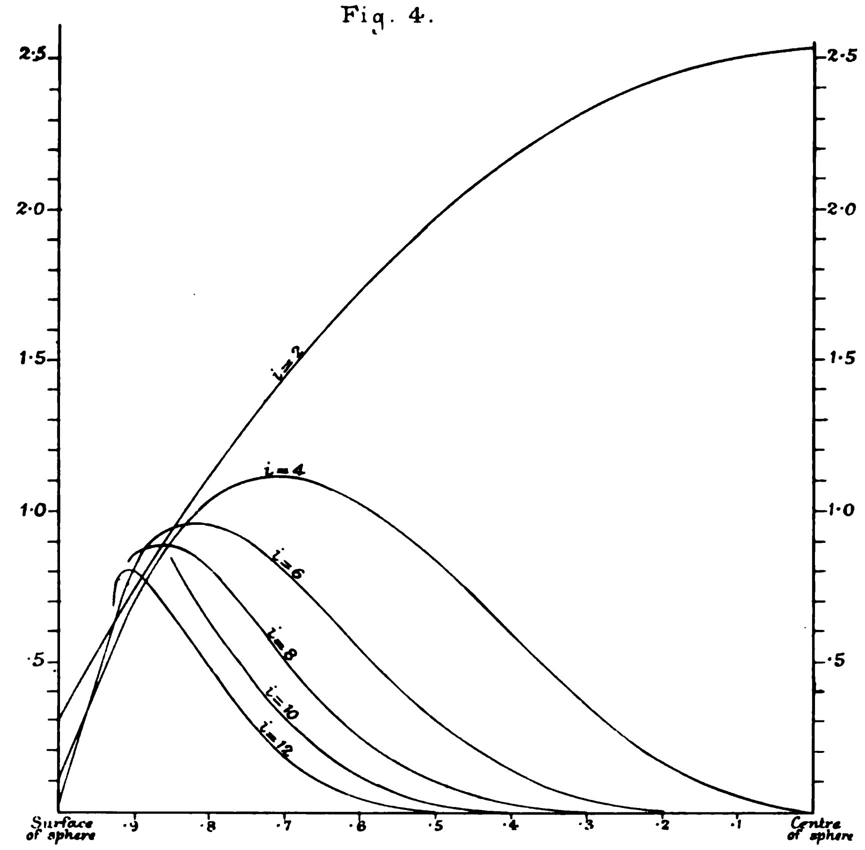


Diagram showing the difference of the principal stresses at the equator, due to inequalities represented by the even zonal harmonics.

Fig. 5.

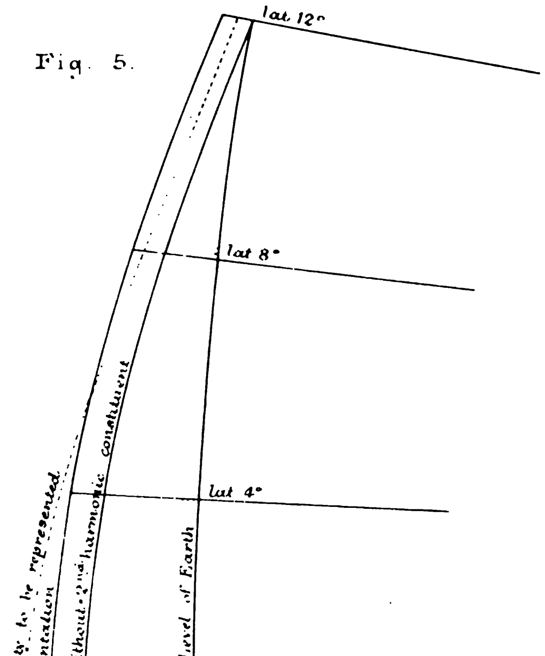

lat 12°
lat 8°
lat 4°
Equator
Inequality to be represented
Representation
Repn without 2nd harmonic constituent
Mean level of Earth

Diagram showing the profile of isolated equatorial continents.
Radius of sphere 18 inches.

Fig 6

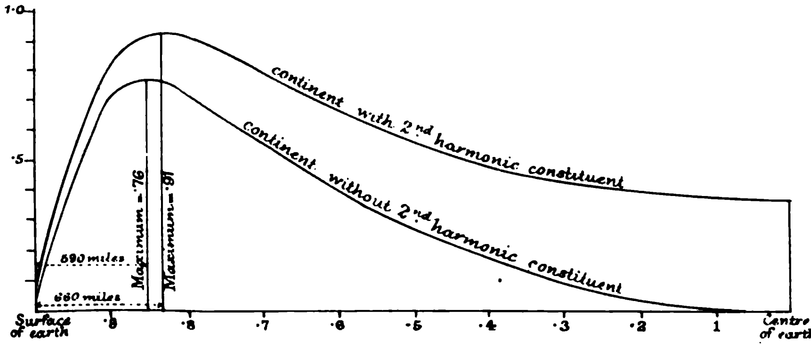


Diagram showing the difference of principal stresses at the equator due to isolated equatorial continents.

West Newman & Co. lith.

Fig. 4.

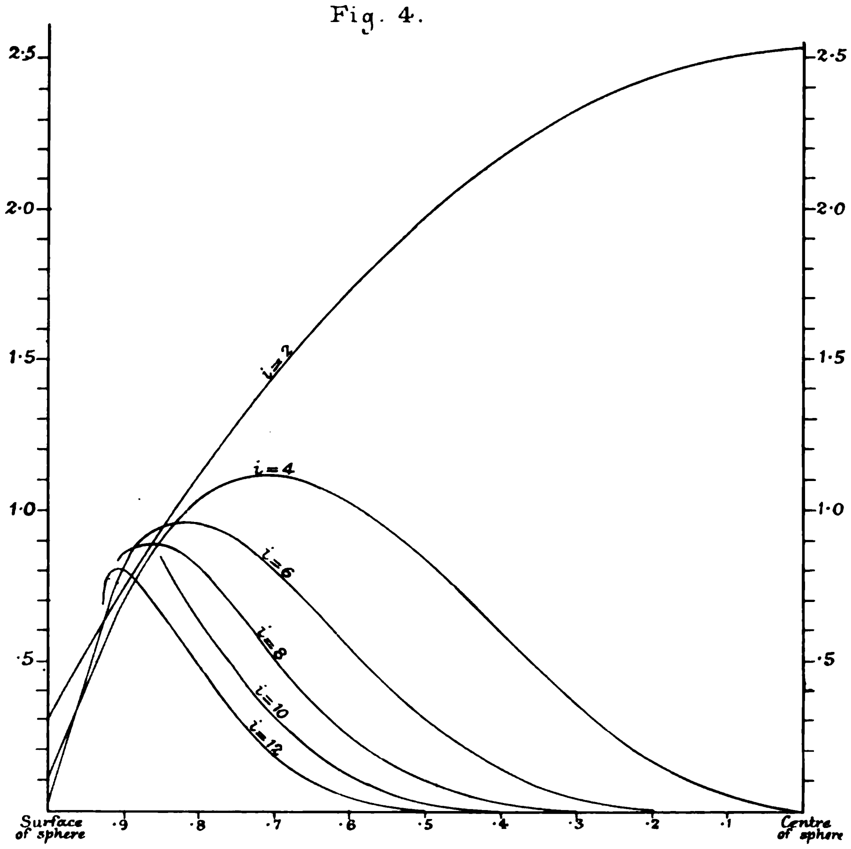


Diagram showing the difference of the principal stresses at the equator, due to inequalities represented by the even zonal harmonics.

Fig. 5.

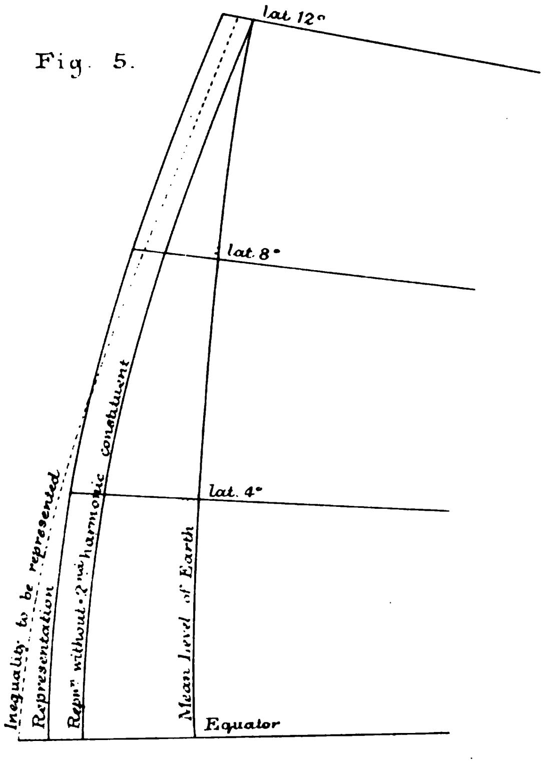

lat. 12°
lat. 8°
lat. 4°
Equator
Inequality to be represented
Representation
Repn. without 2nd harmonic constituent
Mean level of Earth

Diagram showing the profile of isolated equatorial continents.
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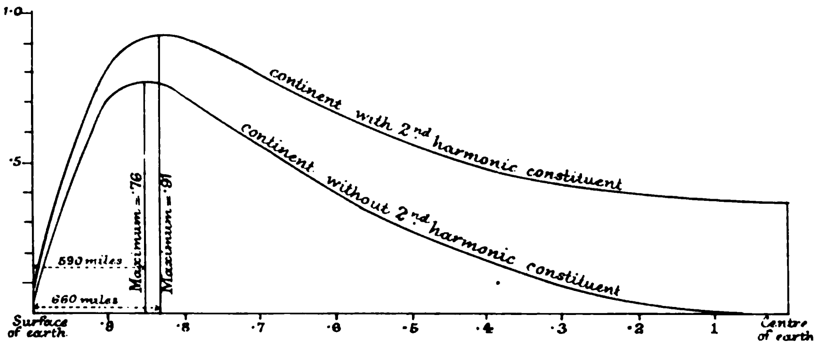

Diagram showing the difference of principal stresses at the equator due to isolated equatorial continents.

West, Newman & Co. lith.